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The professional papers contained in The Journal are published prior to the meetings at which they are to be presented, in order to afford members an opportunity to prepare any discussion which they may wish to present.

SOCIETY HISTORY

CHAPTER VIII

THE WORLD'S FAIR AND THE ENGINEERING CONGRESS

198 At the New York Meeting, held in December 1892, the committee of the Society having in charge the participation of the American Society of Mechanical Engineers in the conduct of the Mechanical Section of the Engineering Congress, to be held at the Columbian Exhibition, reported that the work had been organized by making the officers of Division B of the Congress identical with the officers of the Society. It was announced that the sessions were to be held from July 31 to August 5, 1893, in the Art Palace on the Lake Front in Chicago, the Congress to be opened and closed with general sessions of all the sections, and the intermediate period to be utilized for the special sessions in the different sections in various portions of the building. Under this arrangement papers presented before the Congress relating to mechanical-engineering subjects were to form a portion of the proceedings of the Section, whether by members of The American Society of Mechanical Engineers or by others, the entire proceedings and discussions forming a portion of the published Transactions of the Society.

199 The Committee reported also that the Society had entered definitely into the plan for the maintenance of a joint engineering headquarters in the city of Chicago, thus joining in the plan for the reception and entertainment of visiting engineers from other parts of the world.

200 Other important reports presented at the New York Meeting of 1892 were those upon the testing of steam engines and other machines at the Columbian Exposition, upon flange standardization, and upon the desirability of a standard system for the thickness of metallic plates.

201 The address of the retiring President, Commodore Charles H. Loring, was devoted to the subject of the influence of the steam engine upon modern civilization, showing the enormous influence

which this single product of the engineer had exerted upon the conduct of the world.

202 A number of important papers were presented at this meeting of the Society, among which may be mentioned an analysis of the shaft governor, by Mr. F. M. Rites, setting forth the applications of the inertia principle since so extensively applied to automatic steam engines; a discussion of steam boiler explosions, following the presentation of a paper upon an explosion at Worcester, by Mr. F. H. Daniels, and a very animated discussion upon the subject of the proper qualifications entitling a man to be called an engineer.

203 During this meeting the interest of the members in the approaching exposition was most evident, and the results of the gatherings in Chicago bore out in a most gratifying manner the success which attended the effective preliminary work of the officers, committees, and interested members who realized the importance of the occasion. At this meeting of the Society Mr. Eckley B. Coxe, of Drifton, Penna., was elected President, to serve for the ensuing year.

204 Prior to the gathering of the members for the Engineering Congress at Chicago at the close of July 1893, it became evident that the meeting was to be largely attended and that it was destined to be a great success. Visiting engineers from England, France, Germany, Austria and other countries arrived in New York, either in parties, or individually, and all were received and greeted by the local members, and everything done that was possible to make them realize that the members of the Society appreciated the opportunity which was being offered to return in some degree the hospitalities which had been extended to its members when abroad. The registration at the headquarters at Chicago bore out these indications, the roll including 283 members, besides a large number of guests, including engineers from England, and the British colonies, from France, Belgium, Germany, Austria, Italy, Sweden, Norway, Russia, Japan and South America.

205 Although it was evident that the work of the Congress would naturally occupy most of the time allotted for the convention, it was not forgotten that the meeting was a regular meeting of the Society, and that it should include some of the social features which had become established at such gatherings. The presence of the Exposition offered the great entertainment in itself, but through the effective work of local members some especial opportunities were afforded including a trip to a number of the engineering features of the Exposition, including the construction work of the Exhibition

buildings, the intramural electric railway and its power house, the traveling platform, the extension work upon the Chicago water-works tunnel, and a number of the mechanical portions of individual exhibits not otherwise wholly open to visitors.

206 On the morning of July 31, there was gathered in the Memorial Art Palace a remarkable body of engineers, including large representations from the memberships of the American Society of Civil Engineers, The American Society of Mechanical Engineers, the American Institute of Mining Engineers, and of engineers devoted to military, marine and naval engineering, and to engineering education. In this gathering were included eminent specialists from all parts of the world, men whose names were familiar in the professions and the constructive arts; men upon the results of whose labors depended the manufactures, commerce and industries of civilized countries, who controlled transportation by land and by water, and in whose hands lay the maintenance of peace and the control of national defense. To this assemblage there was made an effective address of welcome by President H. C. Bonney, of the Congress Auxiliary Committee, and after fitting responses from the various delegations there represented, the several sections separated to their assigned meeting rooms for organization and discussion.

207 Following the regular business of the Society, including reports of committees, tellers of election, etc., the effective business of the Congress was begun by the reading and discussion of papers. It is not practicable nor is it desirable to give here any critical analysis of the contributions to the applied science of engineering as presented at this memorable international gathering. It is necessary only to glance at the portly volume of the Transactions of The American Society of Mechanical Engineers for 1893, to realize by its unusual size the extent to which the opportunity was grasped to lay before the assembled members of the profession the latest developments of mechanical engineering. To that volume the interested engineer must be referred for details, and in this place some endeavor will be made to show the important character of the work of the Society as a whole, and its relation to the developments which have followed.

208 One of the most important subjects, measured either by the number of papers or by their relations to subsequent developments was that of locomotive engineering. To this question were devoted papers by Prof. W. F. M. Goss, M. A. Mallet, of Paris, Mr. A. Von Borries, of Hanover, Mr. Albert Schneider, of Brunswick, and

the committee of the Society upon standard methods of conducting locomotive tests. The extent to which these papers and their discussions have influenced subsequent work in their respective lines will be seen when it is perceived that they included studies of locomotive testing apparatus, such as was originally installed at Purdue University, and led to the plant now used by the Pennsylvania Railroad Company at Altoona; the use of articulated locomotives of the Mallet type for maximum tractive power under difficult conditions, since widely employed on both sides of the Atlantic; together with fruitful discussions upon compounding for locomotive engines, and the practical use of rack railways for mountain roads.

209 The subject of steam engineering naturally came in for abundant attention during the Congress. Thus, Professor Dwelshauvers-Dery, of Liége, honorary member of the Society, presented a discussion of the theory of the steam engine, while Mr. Charles T. Porter, honorary member of the Society, discussed the limitations of engine speed, showing the importance of the increase of clearance losses with increased rotative speed. When this paper is taken in connection with that of Mr. Frank H. Ball, upon the fallacy of the assumption that high compression can neutralize clearance losses, it will be seen that some of the essential features of the important discussion of ten years later by Dwelshauvers, Boulvin, Isherwood and others were anticipated. Reports of tests upon pumping engines and upon railway power plants furnished valuable contemporary records, while the posthumous paper of Prof. James H. Fitts referred to the possibilities of the evaporative condenser in a manner which has since been practically realized.

210 Apart from the groups of papers thus referred to, mention may be made of certain isolated communications to the Congress, such as the exhaustive discussion of technical education in the United States, by Dr. R. H. Thurston, upon haulage by horses, by Mr. Thomas H. Brigg, an arraignment of the ordinary defective methods of harnessing draught horses; and upon the development of interchangeable systems of manufacture, by Mr. W. F. Durfee. An interesting anticipation of a comparatively recent development appears in the paper by Herr Pieper, of Hamburg, upon the taximeter for recording fares in public vehicles.

211 A valuable feature of the Congress was the report of the Committee upon Standard Methods of Testing Materials, the secretary of this committee, Mr. G. C. Henning, having attended the conference of the German Union at Vienna in May. The result of

this report was the endorsement by the Congress of the efforts to establish a uniform international system of testing materials.

212 The papers themselves formed but a portion of the real benefit to the profession gained by the participation in the Engineering Congress. At such a gathering the interchange of personal experiences and the opportunity of widening the scope of professional acquaintances is greatly multiplied over the possibilities at an ordinary convention. The Exposition itself formed a remarkable record of the work of the engineer in all departments of effort, and it was possible, within the limits of its grounds, to see much of the best work of the men who themselves were present at the sessions of the Congress. That the close connection of the Society with engineering work at the Exposition was a real stimulus to its growth, was felt by all who had the privilege and opportunity of attending either the Congress or the Exposition or both.

213 The beneficial influence of the activity in connection with the Columbian Exposition was by no means limited to those members who were able to visit Chicago. By far the greater portion of the foreign visitors passed through New York City, and arrangements had been made to extend the use of the house in Thirty-first Street to all who might come; while a special local Committee formed of members of the Council held itself ready to assist in receiving the visitors.

214 Unfortunately it was not found practicable for members of the British engineering societies to visit America in a body, and hence no concerted welcome could be given to them comparable with that which had been given to the American engineers who had visited England four years before. It was therefore possible only to receive the members of the Institution of Civil Engineers and kindred societies individually as they arrived and passed through New York on their way to Chicago, and such opportunities were seized and utilized whenever possible. The French Engineers, however, found it practicable to organize a party of about fifty members of the Société des Ingénieurs Civils de France, who, leaving Havre on August 26, 1893, arrived in New York on September 3, too late to participate in the Engineering Congress, but in ample time to see the Exposition at its best, and to receive the welcome of their former guests from the United States.

215 This party of French engineers, under the guidance of the Marquis de Chasseloup-Laubat, himself a member of the French Society as well as of the French Commission to the Columbia Expo-

sition, had arranged to spend some time in various American cities. Members of The American Society of Mechanical Engineers were at the pier to receive the visitors, and the utmost was done to show them the attractions of New York City during the four days of their stay, while a committee accompanied them on a special train to Chicago, via Niagara Falls. Before returning to New York, the French engineers visited St. Louis, Pittsburgh, Washington, and Philadelphia, and in each of these cities they were received and entertained by members of the Society.

216 At the Annual Meeting of the Society, held in New York in December 1893, it was possible to report upon the entire success of the work of the Society in connection with the Engineering Congress and the World's Fair, and to record the completion of all the business connected with the additional undertaking which it had thus accepted.

OFFSETTING CYLINDERS IN SINGLE-ACTING ENGINES

By Prof. Thurston M. Phetteplace, Providence, R. I.
Member of the Society

A great deal has been said recently about the offsetting of cylinders in single-acting engines and many claims of superiority are made by those who employ this form of construction.

2 About twenty-five manufacturing establishments in the United States are building engines in which the cylinders are offset, chiefly those of the automobile type, and one company is formed for the purpose of making engines in which the offset is equal to the crank radius and the connecting rod length is about $3\frac{3}{8}$ times the crank radius.

3 Among the claims made by manufacturers for offset engines are: greater power, less side-pressure of the piston on the walls of the cylinder, better turning effort, less vibration, smoother running qualities, and when one cam shaft is used, a more convenient mechanical arrangement.

4 On account of the importance of this subject and the lack of information concerning it, a complete discussion is desirable and is here presented.

5 The cylinder of an engine is said to be offset when its center-line is not in a plane through the center of the crank shaft. The practice is not new and is applied to both steam and gas engines having one or any number of cylinders.

6 In the diagram, Fig. 1, AB represents the stroke, OE the crank radius, DE the connecting rod, θ the crank angle, and OC the offset. It should be noticed that θ is the angle the crank makes with a line through the center of the crank shaft parallel to the center-line of the

The full development of the mathematical formulae of this paper, with some other related matter, is given in an unpublished Appendix, which is on file in the Library of the Society for the use of members who wish to verify the mathematical work.

To be presented before The American Society of Mechanical Engineers. All papers are subject to revision.

cylinder, and not the actual angle passed over from the inner dead point. The length of the stroke is

$$AB = R \sqrt{(a + 1)^2 - k^2} - \sqrt{(a - 1)^2 - k^2}$$

which is greater than $2R$.

R = crank radius.
a = L/R.
L = connecting rod length.
k = offset divided by R.

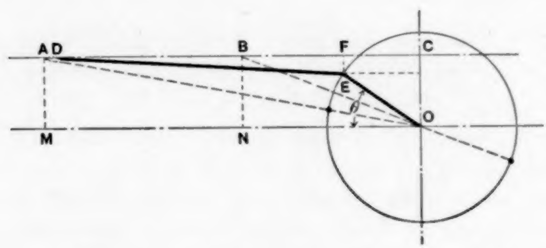


FIG. 1 DIAGRAM OF CRANK AND CONNECTING ROD TRAIN

Thus for 3 in. crank radius the strokes would be as shown in Table 1. The distance to the end of the stroke farther away from the center of the crank shaft is shortened, thus OM is less than $DE + EO$ or $L + R$ which affects the height of the engine.

TABLE 1

Ratio L/R	Offset R	Stroke	Increase Per cent
Any	zero	6.00000	0.00
3	0.10	6.00375	0.06
3	1.00	6.42279	7.04
4	1.00	6.21180	3.50
5	1.00	6.12930	2.15
6	1.00	6.08730	1.01

7 The dead points are not opposite each other, so that the crank angle swept over while the piston makes the out-stroke is less than that for the in-stroke, causing a quick return motion and an average velocity for the in-stroke or compression and exhaust strokes greater than for the out-stroke, or explosion and suction strokes.

8 An expression for the piston position in terms of the crank angle θ is developed in the usual way and is

$$X/R = \sqrt{(a+1)^2 - k^2} - \cos\theta - \sqrt{a^2 - (k\text{-}\sin\theta)^2}$$

in which X = the piston displacement from the end of the stroke farther from the crank shaft.

9 The force of inertia due to the reciprocating parts is equal to the weight multiplied by the acceleration divided by 32.2. The value for the acceleration is found by differentiating the expression for the piston displacement twice with respect to the time. This is done by expanding the radical $\sqrt{a^2 - (k - \sin\theta)^2}$ by the binomial theorem, into a convergent series and then dropping all terms containing a with a negative exponent of 3 or larger in order to get an expression that can be easily differentiated. This gives

$$(a^2 - (k - \sin)^2)^{\frac{1}{2}} = a - \tfrac{1}{2}a^{-1}k^2 + a^{-1}k\sin\theta - \tfrac{1}{2}a^{-1}\sin^2\theta$$

This approximate expression for the radical differs from the radical for different values of k, a and θ, as shown in Table 2.

TABLE 2 DIFFERENCE BETWEEN EXACT AND APPROXIMATE EXPRESSIONS

k	a	θ	$\sqrt{a^2 - (k - \sin\theta)^2}$	$a - \frac{1}{2}a^{-1}k^2 + a^{-1}k\sin\theta - \frac{1}{2}a^{-1}\sin^2\theta$	Difference
1	6	90	6.000000	6.000000	zero
1	6	0	5.916079	5.916666	+.000587
1	6	45	5.992762	5.992867	+.000105
.5	6	90	5.979130	5.979166	+.000036
.5	6	0	5.979130	5.979166	+.000036
.5	6	45	5.996428	5.996429	+.000001
.5	3	90	2.958039	2.958333	+.000294
.5	3	0	2.958039	2.958333	+.000294
.5	3	45	2.992849	2.992859	+.000010
.5	4½	90	4.472136	4.472222	+.000086
.5	4½	0	4.472136	4.472222	+.000086
.5	4½	45	4.495236	4.495239	+.000003

10 The greatest difference has no significant figure until the fourth decimal place is reached and this is when $k = 1$, which is an unusual value. Hence it is readily seen that the error introduced by this approximate form is slight.

11 Substituting this value of the radical in the expression for the piston displacement and differentiating twice with respect to the time gives

$$F/A = 0.00034\ W/A\ N^2R\ (a^{-1}k\sin\theta + \cos\theta + a^{-1}\cos 2\theta)$$

which is the expression for the inertia force per square inch of piston head area when there is an offset.

A = area of piston head.
W = weight of the reciprocating parts.
N = revolutions per minute.
R = crank radius in feet.

This differs from the similar expression when there is no offset by the addition of the term $a^{-1}k \sin \theta$, so that tables for inertia factors for no offset may be used by adding the value of this term.

12 The expression for the tangential pressure or the turning force for any offset is

$$T = P\left(\sin\theta + \cos\theta\,\frac{\sin\theta - k}{a - \frac{1}{2}a^{-1}k^2 + a^{-1}k\sin\theta - \frac{1}{2}a^{-1}\sin^2\theta}\right)$$

in which P is the pressure on the piston pin in the direction of the center of the cylinder. This is a long expression to solve and a graphical solution may be followed if preferred. The work of solving the expressions for inertia force and tangential pressure may be somewhat lessened by tabulating the quantity $a^{-1}k \sin \theta$ which appears in these expressions.

13 The derivation of the preceding formulae and tables is shown in the appendix.

SIDE PRESSURE OF PISTON ON CYLINDER WALLS

14 A reduction of the side pressure of the piston on the cylinder walls is one of the advantages claimed for offsetting.

15 There are two ways in which the side pressure may affect the single-acting engine: (*a*) The maximum value of the side pressure determines the length of piston to keep the maximum pressure per square inch of projected area below a value which is assumed as not too great to destroy the oil film between the rubbing surfaces; (*b*) The average value of the side pressure produces the friction between the sliding surfaces causing a mechanical loss and some wear of the parts. The loss in mechanical efficiency is more important than the wear, especially in the small high-speed automobile engines.

16 The average side pressures may be found by adding all of the areas between the axis and the curve of side pressures and dividing by the total length, or the areas themselves may be taken for comparison, as they represent the work done on the side of the cylinder

by the piston, which is lost work and should be kept as low as possible. Curves of side pressures of the piston on the cylinder walls were constructed, it being necessary (*a*) to assume a gas card, (*b*) to assume engine dimensions, (*c*) to calculate inertia forces and plot curves,

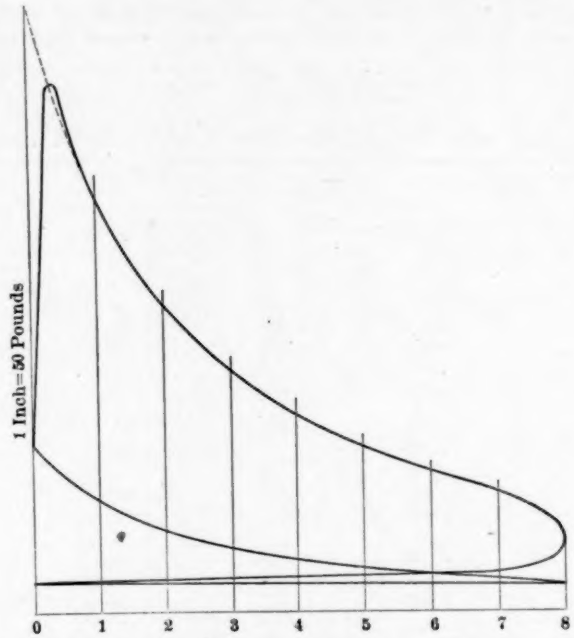


FIG. 2 OFFSETTING CYLINDERS IN SINGLE-ACTING ENGINES
Gasolene Card Compression, 70 lb.; Maximum Pressure, 259 lb.; Pressure ratio, 3.77

(*d*) to combine inertia forces with gas pressures, obtaining the force at the piston pin, and then (*e*) to determine the side pressure component perpendicular to the center-line of the cylinder for the changing angularity of the connecting rod. The gasolene card chosen is shown in Fig. 2. As it seemed desirable to investigate two similar cases, one

TABLE 3

Specifications	Slow	High
R.p.m.	450	1500
W/A....................................	1 lb.	0.70 lb.
R....................................	6 in. = 0.5 ft.	$2\frac{1}{2}$ in. = 0.208 ft.
0.00034 $W/A\ N^2R$	34.4	111.38

for high speed and the other for slow speed, the dimensions given in Table 3 were chosen.

TABLE 4 PISTON POSITION FACTORS

CALCULATED BY THE FORMULA

$$X/R = \sqrt{(a+1)^2 - k^2} - \cos\theta - a + \tfrac{1}{2}a^{-1}k^2 - a^{-1}k\sin\theta + \tfrac{1}{2}a^{-1}\sin^2\theta$$

FROM BEGINNING OF STROKE TOWARDS THE CRANK SHAFT. MULTIPLY BY CRANK RADIUS TO FIND PISTON POSITION. (NOTE: CRANK RADIUS IS NOT ONE-HALF OF THE STROKE)

Crank Angle	$L \div R = 3$		$L \div R = 4\frac{1}{2}$	
	Offset = 0.30 R	Offset = 0.50 R	Offset = 0.30 R	Offset = 0.50 R
4°18′	0	.....	.....	.....
7°11′	.....	0	.....	.....
3°7′	.....	.....	0	.....
5°13′	.....	.....	.....	0
15	0.023	0.012	0.026	0.018
30	0.129	0.103	0.130	0.111
45	0.309	0.269	0.303	0.275
60	0.525	0.491	0.527	0.492
75	0.804	0.746	0.782	0.742
90	1.070	1.010	1.046	1.005
105	1.321	1.263	1.299	1.26
120	1.525	1.491	1.527	1.49
135	1.723	1.683	1.717	1.69
150	1.861	1.835	1.862	1.84
165	1.955	1.944	1.958	1.95
180	2.0037	2.0102	2.0018	2.0049
188°38′	2.0114	......	......	......
194°29′	......	2.0321	......	......
184°55′	......	......	2.0047	......
188°13′	......	......	......	2.0131
195	2.0065	2.0303	1.992	2.007
210	1.961	2.001	1.929	1.954
225	1.865	1.918	1.810	1.846
240	1.699	1.779	1.643	1.684
255	1.515	1.585	1.429	1.475
270	1.270	1.343	1.179	1.227
285	0.997	1.068	0.911	0.957
300	0.699	0.779	0.643	0.684
315	0.471	0.504	0.407	0.432
330	0.229	0.269	0.197	0.222
345	0.075	0.098	0.061	0.075
360	0.0037	0.0102	0.0018	0.0049

17 The piston position factors and inertia factors are given in Tables 4 and 5, and Fig. 3 to Fig. 8 give the curves of inertia forces.

The full lines represent the slow-speed and the mixed lines the high-speed cases. Inertia curves and side-pressure curves were plotted for ratios of L/R of $4\frac{1}{2}$ and 3, and for offsets of zero, $0.30\,R$, and $0.50\,R$ for both high and slow speeds, making twelve cases in all. When there is an offset the inertia curve must be plotted for 360 deg. instead of 180, since for the return stroke it is not the reverse of that for the forward stroke, as is the case when there is no offset.

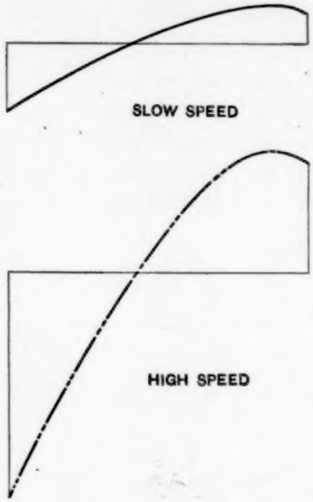
TABLE 5 INERTIA FACTORS

CALCULATED BY FORMULA ($a^{-1}k \sin\theta + \cos\theta + a^{-1}\cos 2\theta$)

Angle	$L/R = 3$		$L/R = 3\frac{1}{2}$	$L/R = 4$	$L/R = 4\frac{1}{2}$		
	$K = 0.30$	$K = 0.50$	$K = 0.20$	$K = 0.30$	$K = 0.30$	$K = 0.40$	$K = 0.50$
15	1.280	1.297	1.229	1.200	1.175	1.181	1.187
30	1.083	1.116	1.037	1.028	1.010	1.021	1.033
45	.778	.825	.747	.760	.754	.769	.785
60	.419	.477	.406	.440	.447	.465	.485
75	.067	.131	.066	.104	.131	.152	.174
90	−.233	−.166	−.229	−.175	−.156	−.134	−.111
105	−.450	−.386	−.451	−.404	−.385	−.364	−.344
120	−.580	−.523	−.594	−.560	−.553	−.535	−.515
135	−.636	−.589	−.666	−.654	−.660	−.645	−.629
150	−.650	−.617	−.695	−.704	−.722	−.711	−.699
165	−.653	−.635	−.703	−.731	−.757	−.751	−.745
180	−.667	−.667	−.714	−.750	−.778	−.778	−.777
195	−.703	−.721	−.733	−.769	−.791	−.797	−.803
210	−.750	−.783	−.752	−.778	−.788	−.799	−.810
225	−.778	−.825	−.747	−.760	−.754	−.769	−.785
240	−.753	−.811	−.692	−.690	−.669	−.687	−.707
255	−.644	−.708	−.561	−.548	−.513	−.534	−.556
270	−.433	−.500	−.343	−.325	−.288	−.310	−.333
285	−.127	−.191	−.044	−.040	.003	.018	−.040
300	.246	.189	.308	.310	.331	.313	.293
315	.636	.589	.667	.654	.660	.545	.629
330	.983	.950	.981	.954	.944	.933	.917
345	1.228	1.211	1.199	1.164	1.141	1.135	1.129
360	1.333	1.333	1.286	1.250	1.222	1.222	1.222

18 Comparing Fig. 3 with Fig. 6 a slight hump is noticed at the right-hand side in the former but not in the latter. This is probably due to error in the formula, for the small value of L/R since the force could not be higher near the end of the stroke than at the end.

19 The general effect of offsetting on the inertia curve is shown in Fig. 9, where the curves for $L/R = 3$, offsets = zero and $0.50\,R$, are compared, the curve for no offset being in full lines.



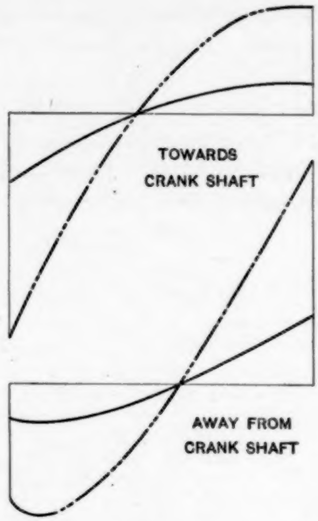
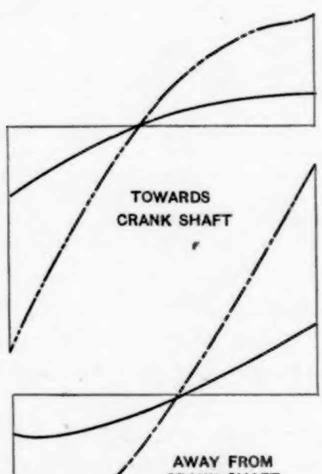


FIG. 3 FIG. 4 OFFSET = 0.30 *R*.

CURVES OF INERTIA FORCES ON PISTON POSITION BASE

Slow Speed: r.p.m. = 450; R = 6; $\frac{W}{A}$ = 1 lb.; L ÷ R = 3.
High Speed: r.p.m. = 1500; R = 2½ in.; $\frac{W}{A}$ = 0.7 lb.; L ÷ R = 3. Offset zero.
Full Lines, Slow Speed; Mixed Lines, High Speed.



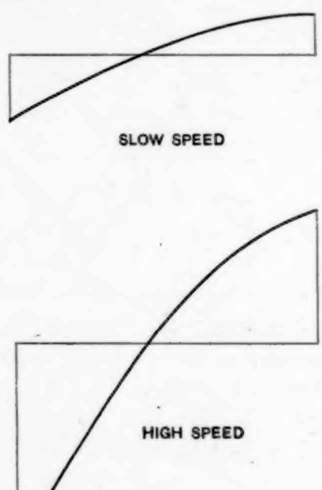
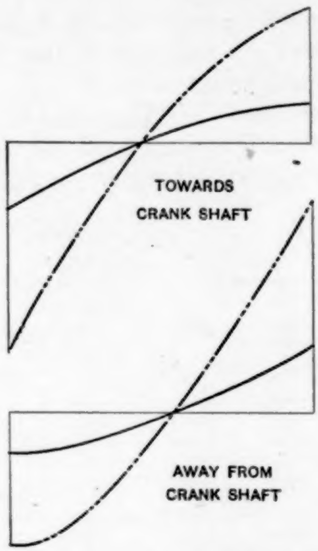


FIG. 5 L ÷ R = 3. Offset = 0.50 R. FIG. 6 L ÷ R = 4½. Offset = zero.

CURVES OF INERTIA FORCES ON PISTON POSITION BASE



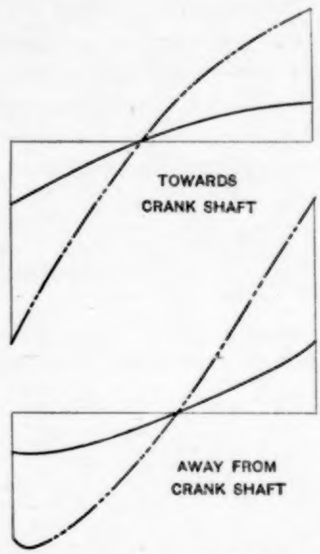


FIG. 7 $L \div R = 4\frac{1}{2}$. Offset = 0.30 R. FIG. 8 $L \div R = 4\frac{1}{2}$. Offset = 0.50 R

CURVES OF INERTIA FORCES ON PISTON POSITION BASE

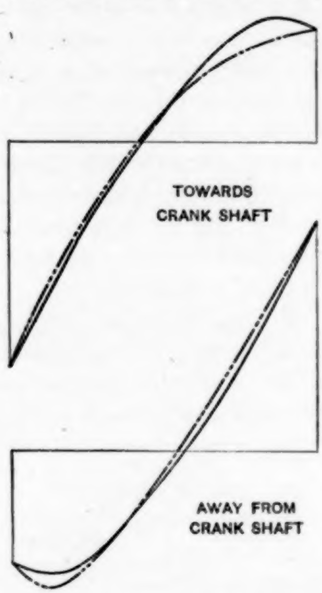


FIG. 9 INERTIA FORCE CURVES SHOWING EFFECT OF OFFSETTING

$L \div R = 3$, High Speed. Full Lines, Offset = Zero; Mixed Lines, Offset = 0.50 R

20 The curve for an offset is a little flatter near the end of the out stroke and the hump is increased near the beginning of the return stroke, which is probably due to inaccuracy in the formula.

21 The curves of side-pressures are shown in Fig. 10 to 15. The maximum side-pressure, its cause (whether combined gas and inertia pressure or inertia force alone) and its location are given in Table 6.

TABLE 6

Ratio L/R	Offset	MAX. SIDE-PRESSURE		DUE TO		STROKE	
		Slow	High	Slow	High	Slow	High
$4\frac{1}{2}$	0	25	26	Gas	Gas	1	1
$4\frac{1}{2}$	0.30 R	17	24	Gas	Inertia	1	2
$4\frac{1}{2}$	0.50 R	12	28	Gas	Inertia	1	2
3	0	$35\frac{1}{2}$	45	Gas	Gas	1	1
3	0.30 R	23	39	Gas	Inertia	1	2
3	0.50 R	20	51	Inertia	Inertia	2	2

22 From this for $L/R = 4\frac{1}{2}$, slow-speed, maximum pressure is lowest with 0.50 R offset, and if the offset were further increased the maximum side-pressure would probably not be reduced as the values at the beginning of the second and fourth strokes would increase, and now they are already 11 so that any increase would soon cause an increase in the maximum value instead of a decrease. In the case of $L/R = 3$ the lowest maximum value occurs when the offset is 0.50 R, but in this case it is possible that the offset is already a trifle large, as the maximum value occurs at the beginning of the second stroke, although it is not much greater than that in the first stroke, being 20 in the former case and 18 in the latter. Hence for the slow speed the best offset would seem to be about 0.50 R as far as the maximum value of side-pressure is concerned.

23 In the case of $L/R = 4\frac{1}{2}$, high speed, the maximum side-pressure due to inertia force at the beginning of the second stroke seems to increase with the amount of offset, while the maximum value due to the gas pressure in the first stroke seems to decrease with the increase in offset. These values are shown in Table 7. $L/R = 4\frac{1}{2}$.

TABLE 7

Offset		Zero	0.30 R	0.50 R
Side pressures due to..........	Gas pressure, 1st stroke...	26	17	13
	Inertia, 2d stroke..........	15	24	28

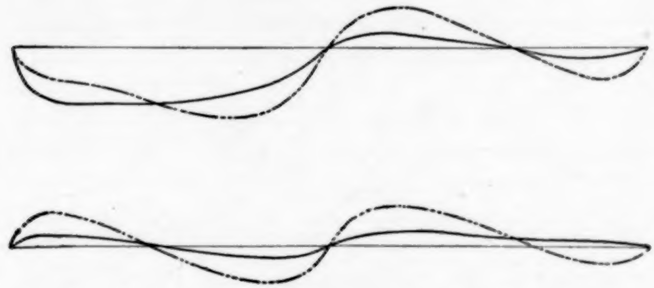


Fig. 10 Curve of Side Pressures on Piston Position Base
C ÷ R = 3, No Offset. Full Lines, Slow Speed; Mixed Lines, High Speed

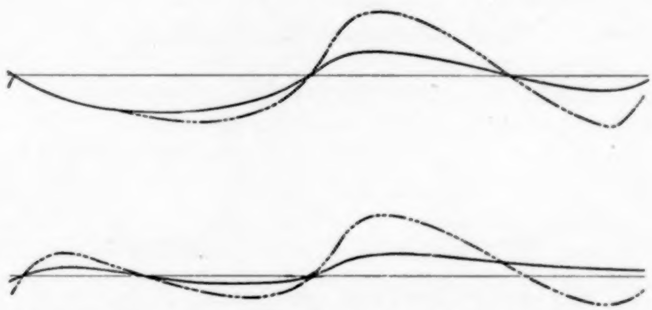


Fig. 11 Curve of Side Pressures on Piston Position Base
L ÷ R = 3, Offset = 0.30 R. Full Lines, Slow Speed; Mixed Lines, High Speed

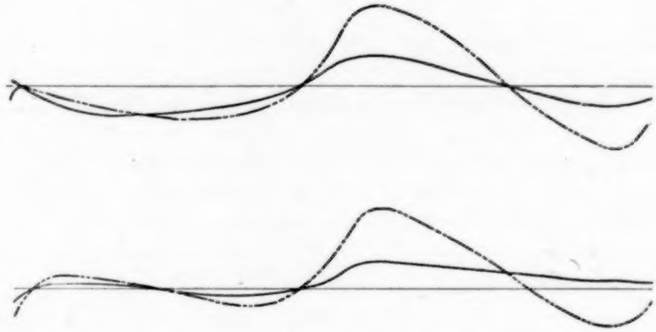


Fig. 12 Curve of Side Pressures on Piston Position Base
L ÷ R = 3, Offset = 0.50 R. Full Lines, Slow Speed; Mixed Lines, High Speed

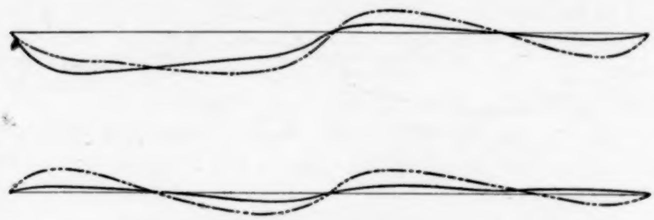


Fig. 13 Curve of Side Pressures on Piston Position Base

L ÷ R = 4½, Offset = Zero. Full Lines, Slow Speed; Mixed Lines, High Speed

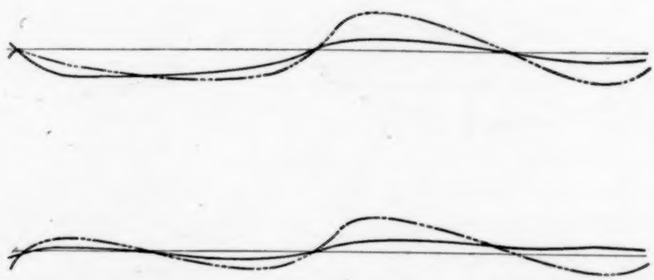


Fig. 14 Curve of Side Pressures on Piston Position Base

L ÷ R = 4½, Offset = 0.30 R. Full Lines, Slow Speed; Mixed Lines, High Speed

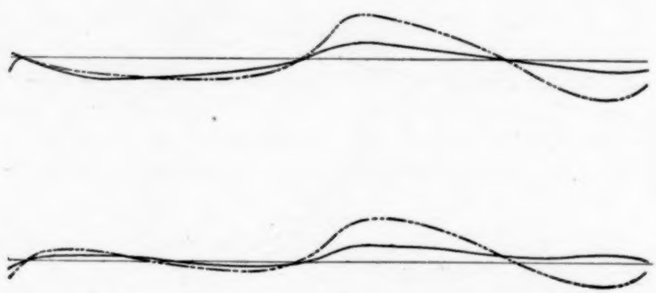


Fig. 15 Curve of Side Pressure on Piston Position Base

L ÷ R = 4½, Offset = 0.50 R. Full Lines, Slow Speed; Mixed Lines, High Speed

24 Plotting curves of these values, the most favorable offset as far as maximum side-pressure is concerned is 0.16 R when $L/R = 4\frac{1}{2}$. This curve is shown in Fig. 16.

25 See Table 8 for values $L/R = 3$. This would place the best offset for $L/R = 3$, as far as maximum side-pressure is concerned,

TABLE 8

Offset		Zero	0.30 R	0.50 R
Side pressures due to.........	Gas pressure, 1st stroke...	45	30	20
	Inertia, 2d stroke..........	25	39	51

as 0.20 R, which would seem to indicate that a greater offset would be desirable as the ratio L/R decreased. It remains to determine if possible the best offset as far as the work done in side-pressure is concerned.

26 The work done is proportional to the areas included between the axis and the curve of side-pressures. It seems to make no difference whether a larger amount of work is done on one side than on the other, or in other words there seems to be no advantage in having the work done on each side the same, unless at some time it might be desired to rebore the cylinder, in which case wear occurring all on one side might have left the walls too thin or might necessitate the removal of much more metal.

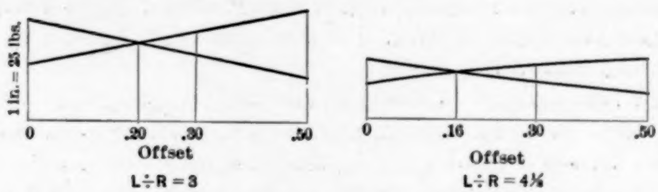


FIG. 16 CURVE SHOWING VARIATION OF SIDE PRESSURE WITH OFFSET

27 Table 9 shows the results of measuring the areas, namely the ratio of work done in one case to that in each of the other cases, and also the actual average side-pressures. For the *slow-speed* case there seems to be a decrease of work done on one side, and an increase of work done on the other side, resulting in a decrease in the total work done with the offset, which would indicate that the greater the offset, the less the loss in work. The average side-pressure also decreases with the offset, although it is less with no offset when $L/R = 4\frac{1}{2}$ than with 0.50 R offset when $L/R = 3$.

28 In the case of the *high speed* the areas on one side decrease with the offset while those on the other side increase, but the totals for $L/R = 4\frac{1}{2}$ decrease and then increase, while for $L/R = 3$ they continue to increase, and of course the same is true for the mean side-pressure.

TABLE 9

Ratio L/R	Offset	SLOW SPEED				HIGH SPEED			
		+ Area	− Area	Total Area	Average Side Pressure	+ Area	− Area	Total Area	Average Side Pressure
$4\frac{1}{2}$	0	1.61	− .53	2.14	6.6	2.33	−1.11	3.44	10.7
$4\frac{1}{2}$	0.30 R	1.09	− .90	1.99	6.2	1.78	−1.58	3.36	10.5
$4\frac{1}{2}$	0.50 R	0.74	− .93	1.67	5.2	1.62	−1.90	3.52	11.
3	0	2.59	− .92	3.51	11.	4.11	−1.45	5.56	17.3
3	0.30 R	1.55	−1.15	2.70	8.4	3.01	−2.76	5.77	18.0
3	0.50 R	1.11	−1.39	2.50	7.8	2.47	−3.40	5.87	18.3

29 This would seem to indicate that there is little if anything to be gained by an offset in regard to work done by the piston on the walls of the cylinder when the inertia force is very high, since what is gained on one side is more than made up in loss on the other side.

30 If it is of sufficient importance to have the work done on each side of the cylinder the same, we may plot curves of the work done on each side and note where they intersect, as in Fig. 17 . In the case of the slow speed we would have the work done on each side equal when the offset was about 0.40 R and in the high speed this point would be about 0.36 R.

31 *Thermal Cycle.* Offsetting increases the length of stroke, which gives increased expansion to the gas, and increases the piston velocity on the in-stroke, giving greater inertia to the gas on the exhaust and reducing the amount of leakage by the piston on the compression stroke. This refers to the 4-cycle gas engine.

32 *Lubrication.* The curves of side-pressure show the manner in which the side-pressure changes sides, which is a good thing for lubrication. This changing sides would be about the same for offset or no-offset except in the case when the offset is equal to the crank radius. Here the pressure is almost continually on one side of the cylinder so that oil would with difficulty be introduced between the surfaces. Other things being even, except for this extreme case, the reduction in amount of side-pressure should make lubrication more satisfactory.

33 *Vibration and Balance.* Revolving masses and reciprocating masses may cause vibration in gas engines. Offsetting the cylinders would not affect the revolving masses at all but does change the curves of inertia forces, as already shown in Fig. 3 to 8. These inertia-force diagrams are now combined in different ways according to different arrangements of cylinders, and are compared with similar curves when there is no offset.

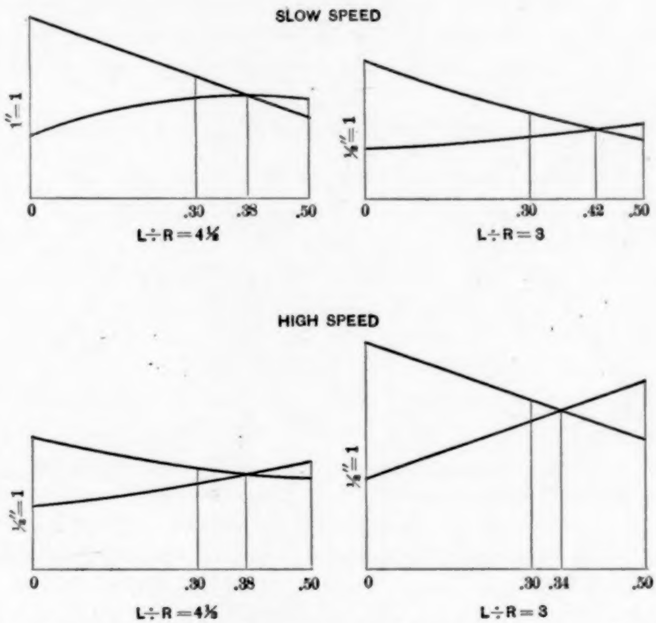


Fig. 17 Curves Showing Offset when Work is same on Each Side of Cylinder

34 The following discussion applies only to the 4-cycle type of gas engine, whose arrangements are:

a Single cylinder.
b Two-cylinder vertical.
c Two-cylinder opposed.
d Three-cylinder vertical.
e Four-cylinder vertical.
f Four-cylinder double-opposed.
g Six-cylinder vertical.

35 For this comparison the high speed case, when $L/R = 4\frac{1}{2}$, was chosen, the offset being equal to zero and one-half the crank radius.

36 Fig. 18 shows the inertia curves for a single-cylinder engine. These curves must be shown for 360 deg. of crank angle, for they differ on the return and forward strokes. The curves for no-offset are shown in full lines and for 0.50 R offset in dotted lines. The difference between the two curves is apparent.

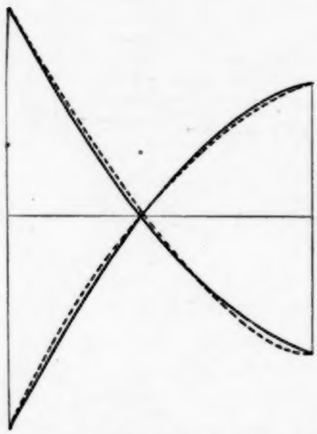


FIG. 18 CURVES OF INERTIA FORCES ON PISTON POSITION BASE

L ÷ R = 4½. Full Line, Offset = Zero; Dotted Line, Offset = 0.50 R. High Speed Case

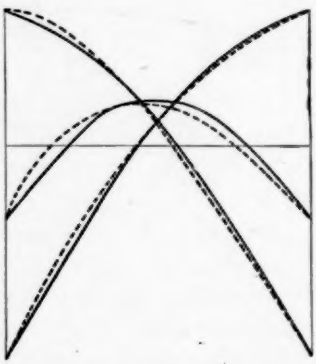


FIG. 19 CURVES OF FREE UNBALANCED INERTIA FORCES. TWO CYLINDER VERTICAL ENGINE

L ÷ R = 4½; Full Line, Offset = Zero; Dotted Line, Offset = 0.50 R. High Speed Case

37 Fig. 19 shows the curve of free inertia forces for a two-cylinder vertical arrangement. These curves are not so very different; the one for an offset being nearly the same as the other but moved along a little instead of being symmetrical with a center line perpendicular to the axis. The maximum values of the forces are about the same. The vibrations when there is an offset would have unequal periods but about the same amplitudes. For the four-cylinder vertical case the ordinates of these curves could be doubled and the same general difference would exist.

38 In the case of the two-cylinder opposed motor with cranks at 180 deg., the inertia forces would be balanced whether the cylinders were offset or not, but in the case of an offset a new couple in

a plane perpendicular to the axis would be introduced due wholly to the offsetting, which cannot be balanced. The couple in an axial plane due to the cylinders being not in line would be the same, offset or not, but with an offset there would be added another couple in this plane due to the offset, which would not be balanced.

39 In the case of a four-cylinder double-opposed motor the forces would be balanced and also the couples in an axial plane, but the couples in the plane perpendicular to the axis would be doubled while those in the axial plane due to the offset would be balanced.

40 The case of a three-cylinder vertical arrangement can be discussed by considering the formula for the inertia forces,

$$F/A = 0.00034\ W/A\ N^2R\ (a^{-1}k \sin\theta + \cos\theta + a^{-1}\cos 2\theta)$$

Let the cranks be at 120 deg.; then the crank angles will be θ, $\theta + 120$, and $\theta + 240$. Substituting these values in the formula, the part in brackets reduces to zero, showing that the inertia-forces are balanced. However, the moments resulting from these forces are not balanced. By placing two three-cylinder vertical engines together so that the two middle cranks are in the same plane the six-cylinder engine is obtained, in which the inertia forces and couples are both balanced.

41 From this discussion it follows that offsetting the cylinders has no effect on the vibration due to the reciprocating parts, except in the case of the 2-cylinder opposed and 4-cylinder double-opposed arrangements of cylinders. In these cases the offsetting increases the unbalanced inertia-force couples by adding new ones.

42 Vibration may be felt from the irregularity of the turning-effort curves, which for four different cases are shown in Fig. 22. There is such a slight difference here that it can be neglected, especially since the turning-effort curve depends so directly on the shape of the gas card, which may vary considerably. The conclusion in regard to vibration would be that offsetting does not affect the vibration appreciably except in the case of a two-cylinder opposed or a four-cylinder double-opposed motor.

GENERAL CONCLUSIONS

43 The following are perfectly general conclusions, to be followed by a more definite comparison of actual engines.

a The length of stroke for a given crank radius increases as the offset increases.

b The length of stroke for a given crank radius for any offset decreases as the ratio of L/R increases.

c The increase in length of stroke causes an increase in average piston speed.

d Offsetting the cylinders makes the crank and connecting rod train a quick return mechanism.

e When the cylinders are offset the crank passes over an angle greater than 180 deg. during the out-stroke of the piston, and less than 180 deg. during the in-stroke.

f The average velocity of the piston is greater on the exhaust and compression than on the explosion and suction strokes.

g Offsetting the cylinders reduces the angularity of the connecting rod on the out-stroke and increases it on the instroke.

h When there is an offset, the side-pressure of the piston on the cylinder walls does not change sides at the end of the stroke, but just after the beginning and just before the end of the out-stroke.

i The place where this change of side-pressure occurs approaches the middle of the stroke as the amount of offset approaches the crank radius.

j With no offset, high inertia forces do not greatly increase the maximum side-pressure during the explosion stroke, but do increase it considerably, during all of the other strokes, and this effect is slightly greater as the ratio of L/R decreases.

k With no offset the work done increases with the inertia-force and as the ratio of L/R decreases.

l For low inertia forces, as far as the maximum value of side-pressure is concerned the best offset is one-half the crank radius.

m Considering the maximum value of the side-pressure only, the most favorable value for the offset decreases as the inertia-forces increase, for any ratio of L/R, but does not decrease as rapidly, for smaller values of the ratio L/R.

n For low inertia forces, the work done by the piston on the cylinder walls decreases as the offset increases, but of course is greater for smaller values of L/R.

o For very high inertia forces, the work done decreases slightly with the offset up to 0.40 of the crank radius for value of $L/R = 4\frac{1}{2}$.

p For very high inertia forces, and small values of L/R, there is no advantage in an offset, as far as the work done by the piston on the cylinder walls is concerned.

q The thermal cycle is slightly benefited by offsetting and the benefit increases with the amount of offset.

r Lubrication should be slightly improved by offsetting the cylinders.

s Vibration due to the free inertia forces is no different except in the case of a two-cylinder opposed or four-cylinder double-opposed motor.

44 Table 10 gives data of gas engines that have been constructed and put in operation. The average crank radius is about 2½ in., the

TABLE 10 DATA OF GAS ENGINES HAVING CYLINDERS OFFSET

No.	R Crank Radius	L Length of Connecting Rod	Ratio L/R	Offset Amount	Offset Per cent	Length of Piston	Diam. of Bore	Ratio Piston Length to Diameter
1	7	23⅝	3.37	7	100	14	9.47	1.47
2	2 9/16	8 15/16	3.48	⅝	24.	6	5½	1.09
3	2¼	8½	3.77	⅜	16.6	6	4	1.5
4	2½	9½	3.8	⅜	15	5⅝	5	1.125
5	2½	10	4.0	⅜	15	6½	4¾	1.37
6	3	12¼	4.08	9/16	18.75	5⅝	....	....
7	2⅜	9¾	4.1	½	21	5⅜	5	1.075
8	2½	10½	4.2	⅞	35	5 7/32	4¾	1.098
9	2¾	12	4.36	15/16	34	6¾	5¼	1.28
10	2⅜	10⅜	4.36	⅞	38	5½	4¼	1.29
11	2½	12	4.8	1	40	6	4¾	1.26
12	2½	12	4.8	1	40	6¼	5	1.25
13	2½	12	4.8	1	40	6	4¾	1.26
14	2½	....	....	1¼	50	....	....	....
15	2¾	....	....	1⅛	40	....	....	....
16	3	....	....	1	33	....	....	...
17	2½	....	....	¾	30	....	....	....

Westinghouse standard engine has an offset of 50 per cent of crank radius.

ratio of L/R varies from 3.48 to 4.8 and the percentage of offset varies from 15 to 50. The average diameter of cylinder-bore is 4.81 in. and the average ratio of length of piston to diameter is 1.24.

45 For comparison of engines the following dimensions were taken:

Crank radius	= $2\frac{1}{2}$ in.
Diameter of bore	= $4\frac{3}{4}$ in.
R.p.m.	= 1000

Weight of reciprocating parts per square inch of piston head area = 0.6 lb.

Ratios of $L/R = 3\frac{1}{2}$, 4, and $4\frac{1}{2}$ and an offset, for each of the values of L/R, the largest amount practicable. These offsets are:

$L/R = 4\frac{1}{2}$	Offset = zero
" $= 4\frac{1}{2}$	" $= 0.40\ R$
" $= 4\frac{1}{2}$	" $= 0.30\ R$
" $= 3\frac{1}{2}$	" $= 0.20$ R

46 Tables were prepared for each of the above cases, and values calculated for crank angles varying by increments of 15 deg. each. Each of these tables contained values for the crank angle, piston position factor, the actual piston position, the gas pressure, inertia factor, inertia force, piston pin pressure, tangential factor, and the turning force from which the inertia curves and turning effort curves were plotted.

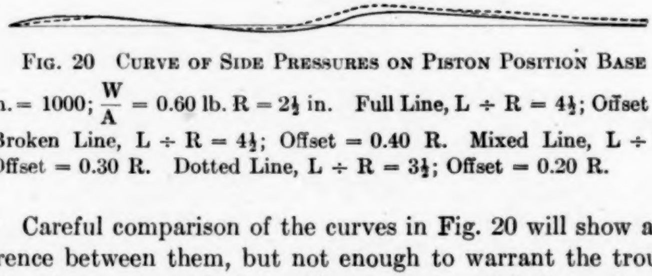


Fig. 20 Curve of Side Pressures on Piston Position Base

R.p.m. = 1000; $\frac{W}{A} = 0.60$ lb. R = $2\frac{1}{2}$ in. Full Line, L ÷ R = $4\frac{1}{2}$; Offset = Zero Broken Line, L ÷ R = $4\frac{1}{2}$; Offset = 0.40 R. Mixed Line, L ÷ R = 4; Offset = 0.30 R. Dotted Line, L ÷ R = $3\frac{1}{2}$; Offset = 0.20 R.

47 Careful comparison of the curves in Fig. 20 will show a slight difference between them, but not enough to warrant the trouble of plotting them separately for use in connection with the gas pressures to find the piston-pin forces from which the side-pressures are determined.

48 The inertia forces shown in Fig. 20 were combined with the gas pressures and the curves of side-pressures plotted as before, with the results shown in Fig. 21.

49 The maximum values for the side-pressure were determined and the areas representing the work done by the piston on the cylinder walls were carefully measured and recorded (see Table 11). As far as these quantities are concerned, the best value is $L/R = 4\frac{1}{2}$, offset = 0.40 R. The turning-effort curves (shown in Fig. 22) are so nearly alike that the difference is hardly worth mentioning.

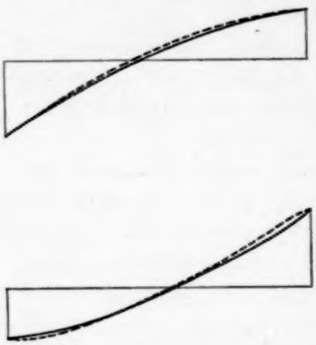


FIG. 21 CURVES OF INERTIA FORCES ON PISTON POSITION BASE

R.p.m. = 1000;

$R = 2\frac{1}{2}$ in.; $\frac{W}{A} = 0.60$ lb.

1 L ÷ R = 4½ Offset = Zero
2 L ÷ R = 4½ Offset = 0.40 R
3 L ÷ R = 4 Offset = 0.30 R
4 L ÷ R = 3½ Offset = 0.20 R

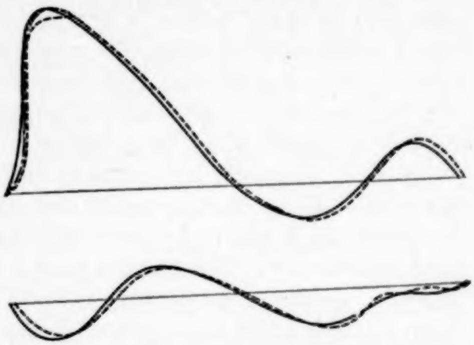


FIG. 22 TURNING EFFORT CURVES ON CRANK ANGLE BASE

Full Line, L ÷ R = 4½ Offset = Zero
Mixed Line, L ÷ R = 3½ Offset = 0.20 R
Dotted Line, L ÷ R = 4½ Offset = 0.40 R
Broken Line, L ÷ R = 4 Offset = 0.30 R

TABLE 11

L/R	Offset	MAX. SIDE PRESSURE		WORK DONE IN AREA UNITS		
		One Side	Other Side	One Side	Other Side	Total
4½	zero	24	7	1.55	0.60	2.15
4½	0.40	12	12	0.93	0.87	1.80
4	0.30	17	12	1.17	0.82	1.99
3½	0.20	23	12	1.53	0.81	2.54

50 Table 12 gives a comparison of the four cases chosen. This table explains itself, but a short discussion may bring out the important points more clearly.

51 There is a slight increase in the length of stroke, but less than one-half of one per cent, so that it amounts to very little. The angle passed over by the crank during the out-stroke is slightly greater than 180 deg. and the greatest gain is 1.7 per cent, which is small. The first great difference occurs in the length of connecting rod, No. 4 effecting a saving of 2.50 in. or 22.2 per cent.

52 Referring to the next line, the distance from the center of the crank-shaft to the position of the center of the piston pin at the end of the stroke, is a measure of the height of the engine and shows a decrease corresponding to the value of L/R.

53 The maximum side-pressure decreases with the offset and increases with the decrease in value of the ratio L/R, so the best case would be No. 2, where L/R is largest and the offset is also largest. Here a reduction of 50 per cent is gained, which reduced the necessary length of the piston 44 per cent. No. 4 is the worst case, L/R very small and the offset also small and then the side-pressure is a trifle less than it is with no offset. The maximum value of the side-pressure affects the length of the piston and consequently the height of the engine, and the length of the cylinder, and so the weight of the cylinder and engine, and the weight of the piston and the corresponding weight of the reciprocating parts, and so the inertia force. The length of the piston is reduced 43.7 per cent in No. 2, 24.4 per cent, in No. 3 and 3.6 per cent in No. 4. The ratio of length of piston to diameter is rather small in No. 2 but is not unusual in the other cases.

54 If it is not desired to take advantage of the maximum value of the side-pressure by reducing the length of the piston, it can be made 1.20 times the diameter, a usual value as is seen in Table 10, which would reduce the pressure per square inch of projected area and so increase the chances of satisfactory lubrication. The reduction of this pressure per square inch of projected area is shown in the next row.

55 In order to find the exact resulting height of the cylinder up to the top of the piston at the end of the in-stroke it is necessary to calculate the position of the piston pin in the piston. This is done in the next row, by making the sums of the products of the areas with the distance from the piston pin center to their centers balance on each side of the piston pin. In case No. 2, with $L/R = 4\frac{1}{2}$, a 50 per cent offset might have been used without interference and this would give better results than 0.40 R offset, but in case No. 4, 0.20 R is undoubtedly about as much as could be used although it would be desirable to use more if a very low engine were wished for.

TABLE 12 COMPARISON OF ACTUAL ENGINES

Specifications	No. 1	No. 2	Gain	No. 3	Gain	No. 4	Gain
Diameter of Bore	2½	2½		2½		2½	
Crank Radius	4¾	4¾		4¾		4¾	
R.p.m.	1000	1000		1000		1000	
W/A	.60	.60		.60		.60	
Ratio L/R	4½	4½		4		3½	
Offset	zero	.40 R		.30 R		.20 R	
Length of Stroke	5.000	5.021	0.42%	5.015	0.30%	5.009	0.18%
Crank Angle passed over, Stroke toward Crank Shaft	180°	182.4°	1.30%	183.08°	1.7%	182.03°	1.1%
Crank Angle passed over, Stroke away from Crank Shaft	180°	177.6°		176.92		177.97°	
Length of Connecting Rod	11.25 in.	11.25 in.		10.0 in.	1.25	8.75 in.	2.50 in.
Distance. Center of Crank, Shaft to end of Stroke	13.75 in.	13.693 in.	.057 in. 0.41%	12.477 in.	1.273 in. 9.2	11.239 in.	2.51 in. 18.2%
Maximum Side Pressure: One Side	24	12	50%	17	29%	23	4.19%
Maximum Side Pressure: Other Side	7	12		12		12	
Piston Length; 15 lb. per sq. in. proj. area; ¾ in. for rings, etc.	6.85 in.	3.86 in.	2.99 in. 43.7%	5.18 in.	1.67 in. 24.4%	6.60 in.	.25 in. 3.6%
Ratio: Piston Length to Diam.	1.44	.81		1.09		1.39	
Pressure per sq. in. proj. area Piston Length = 1.2 diam = 5.7 in	15.7	7.8	50%	11.3	28.1%	15.0	4.45%
Distance: Piston Pin to back end of piston	3.10 in.	1.68 in.		2.29 in.		2.98 in	
Distance: Crank Shaft to end of piston, end of in-stroke	17.5 in.	15.87 in.	1.63 in. 9.3%	15.36 in.	2.14 in. 12.2%	14.86 in.	2.64 in. 15.1%
Length of cylinder, end of piston in-stroke to other end of piston out-stroke	11.85	8.88 in.	2.97 in. 25.1%	10.19 in.	1.66 in. 14%	11.61 in.	.24 in. 2.0%
Work done by Piston on side of Cylinder, square inches	2.15	1.80	.35 16.6%	1.99	.16 7.4%	2.54	−.39 −18.1%
Average side-pressure	6.4	5.6		6.2		7.9	

56 The next two rows show the distance from the center of the crank-shaft to the end of the piston at the end of the in-stroke, which is a measure of the height and the length of the cylinder and also of the weight of the cylinder. As regards the height a gain of 15 per cent may be had in No. 4. No. 2 gives the shortest cylinder, 25 per cent shorter than No. 1, while No. 4 gives one only 2 per cent. shorter. It must be borne in mind that these values are for 1000 r.p.m. and that the value of the maximum side-pressure will increase with the speed. However, the low value of the pressure per square inch of projected area, 15 lb., allows a considerable increase before a dangerous value is reached.

57 The total amount of lost work is shown in the next column. No. 2 gives the best value, a saving of 16 per cent, while No. 4 gives a loss of 18 per cent.

58 In working out a satisfactory solution it would seem that one of two predominating ideas should be followed. Either a very low engine should be aimed at in which everything is sacrificed to height, or else the important object is to reduce to a minimum the side-pressure and the work lost due to friction resulting from side-pressure.

59 In the first case, let $L/R = 3\frac{1}{2}$, offset as much as possible without interference, and a reduction in height of 13 to 15 per cent may be had. This means a reduction and a saving in weight of the connecting rod, cylinder, valve stems, exhaust pipes, inlet pipes, and piston. The actual saving in length in the case above is $2\frac{5}{8}$ in. There will be some increase in the work lost in friction due to the increased average pressure of the piston on the cylinder walls.

60 If a reduction in height is not of primary importance, then a ratio of $L/R = 4\frac{1}{2}$ and an offset of 0.40 R to 0.50 R would seem to give the best results. This gives a reduction in total height of 8 or 9 per cent, a reduction in piston length of 44 to 45 per cent, a reduction in cylinder length of about 20 per cent, and a saving in lost work of about 16 per cent. These reductions would cause a further reduction in weight of piston, weight of cylinder, weight of valve stems, weight of exhaust and inlet manifolds, and a reduction of inertia effects as well as an increased life to the piston, piston rings and cylinder. In this case it might not be desirable to take full advantage of the reduction in length of piston, making it less than the stroke because the oil hole in the side of the cylinder, if one were used, would be uncovered at one end of the stroke or the other.

61 In concluding this comparison, the most desirable offset seems to be as much as can be practically obtained with ratios of $L/R = 4$

and greater, with a decided gain over an engine with no offset for speeds less than 1400 to 1500 r.p.m. The subject may be summed up as follows:

Improvements due to offsetting, (1) in the thermal cycle, (2) in the mechanical arrangement, (3) in the turning effort curve, and (4) in lubrication, are very slight and may be neglected.

62 The real advantages are:

a A reduction of the frictional losses due to the pressure of the piston on the walls of the cylinder, resulting in a slight increase in mechanical efficiency and less wear of the piston, piston rings, and cylinder, and consequently longer life.

b A reduction of the maximum value of the side-pressure of the piston on the walls of the cylinder, allowing the use of shorter connecting rods, shorter pistons, and shorter cylinders, resulting in a shorter and lighter engine and in lower inertia-forces due to the reciprocating parts.

The most important of these advantages would be a considerable saving in weight.

63 The disadvantage of offsetting lies in the fact that the reductions in average side-pressure and maximum side-pressure grow less as the speed and inertia-force increase, so that for a speed of 1400 to 1500 r.p.m. there is either no reduction at all or an increase.

Principal Conclusions

64 Offsetting increases slightly the length of stroke and the crank angle passed over during the stroke toward the crank shaft.

65 The maximum value for the side-pressure of the piston on the cylinder walls decreases as the offset increases up to a value of one-half the crank radius for any ratio of L/R.

66 The work lost in friction due to the side-pressure of the piston on the cylinder walls decreases as the offset increases up to a value of 75 per cent of the crank radius.

67 Both the maximum value of the side-pressure and the work lost in friction increase as the value of the ratio L/R decreases.

68 Offsetting decreases the height and weight of the engine.

69 Offsetting increases the life of the cylinder and piston.

70 Offsetting improves the thermal cycle.

71 The turning-effort curves when the cylinders are offset differ but slightly from those for no-offset.

72 The advantages of offsetting as regards the maximum side-pressure and work lost may be zero or negative for high inertia-forces resulting from speeds of 1500 r.p.m. or more.

DISCUSSION

TRAINING WORKMEN IN HABITS OF INDUSTRY AND COÖPERATION

By H. L. Gantt, Published in The Journal for Mid-November

ABSTRACT OF PAPER

Until within a few years the mechanic was necessarily the source and conserver of industrial knowledge, and on him rested, therefore, the responsibility for training workmen.

With the advent of the scientifically educated engineer capable of substituting a scientific solution of problems for the empirical solution of the mechanic, the responsibility of training workers naturally shifts to his shoulders. It he accepts this responsibility, and bases training on the results of scientific investigation, the efficiency of the workman can be so greatly increased that the manufacturer can afford to give those that take advantage of this training compensation far in excess of that usually paid for similar work.

DISCUSSION

Dr. Alex. C. Humphreys. It has been said that Americans are interested in education only as they can coin it. It is apparent that the educational methods described in Mr. Gantt's able and instructive paper can be coined; but it is encouraging to see the stress laid by the author upon the ethical influence of the methods he describes; and I venture to believe that, if this system were generally introduced throughout the United States, the resulting moral uplift would attract more attention than the increase in dividend-earning capacity.

2 Mr. Gantt refers to the complaint often made as to the growing inefficiency of labor. The complaint is well-founded, but certainly the responsibility cannot rest upon the working class alone. What has been done to meet the demand for trained workers, men

and women, in connection with the radical changes introduced into our industries since the days of the old apprentice system? Some well-directed and successful, but isolated, schemes have been inaugurated. Considerable attention is now being paid to industrial education, and the methods of the author should receive careful attention in this connection, as a substitute for the old apprentice system practically discarded under modern industrial developments.

3 As a people, we are inclined to be superficial. This is true as to much of our educational work, and this system is certainly promising as a corrective. The masses are not to be delivered from the curse of superficiality by ethical arguments; there must be direct and personal influence. It is not only the right, but the duty of all, to make every fair effort to increase their efficiency as wage-earners. Mr. Gantt refers to a force not sufficiently recognized which tends to encourage the worker to acquire greater proficiency and capacity as an earner—the pleasure and pride experienced by the performer in work well done. This is a force we cannot afford to ignore.

4 Our youths are not sufficiently taught the all-important lesson of obedience. Too often liberty degenerates into license; especially is this noticeable in those coming from foreign lands where they have had to endure oppression. We may then well welcome any system which promises systematically to teach obedience to the employed, while keeping the employer constantly reminded that fair play is the price of loyal and efficient service.

5 In this country, we need practical educational methods far more than is generally realized. We flatter ourselves that we are a practical people. If so, why do we not systematically train the youth of both sexes in our public schools to be self-supporting or self-sustaining units of the community? Mr. Gantt advocates teaching the workman, at the same time, *how* and *to do*. A more valuable lesson could not be taught in a country where the people aim to govern themselves.

6 The high degree of efficiency developed by this system is due to the elimination of lost motion. As a people, high and low, not only are we handicapped by the lost motion so generally in evidence, but, in boasting of our smartness, nimbleness and readiness, we fail to recognize the lost motion so often involved in the hasty action and lack of foresight typical of our people.

7 I can well believe that the system advocated by Mr. Gantt can be introduced in many of the factories of the United States to the advantage of the owners and the country at large.

Mr. H. V. R. Scheel. This paper contains many statements which must seem to the practical man and the engineer but expressions of what has been known for a long time, although perhaps not fully realized. Many of them are almost axiomatic. I think no one can doubt that such a system of management must result in the greater efficiency of workmen, individually and collectively, and in the greater coöperation of workmen, foremen and managers, with the attendant economies.

2 The history of mechanical development shows that inventions and improvements by men of no great mental training were until very recently more numerous than by men with trained minds. A possible reason is that men of the former class were intimately acquainted with the operation of the machine; whereas the attention of the latter has been claimed by what they, at least, considered larger and more important matters. Our mechanical and industrial advance would have been more rapid had investigations of individual operations been made by men better fitted for the work. The best methods should have been determined and the workmen trained in those best methods with the spur of increased earnings.

3 Before the days of the corporation, the factory system and the highly specialized workman, a very large proportion of workmen worked for themselves, and since in the last analysis the main reason for action is self-interest those men worked more industriously, more intelligently and with less waste of all kinds than the present-day workman. A bonus determined in a scientifically correct manner seems to be the best appeal to an individual's self-interest: however, a scientific determination does not mean that the employer is entitled to more than his fair share of the savings made.

4 Under this system all the men are pushers to the limits set by the men responsible for quality. The gang bosses and foremen who were formerly drivers are now principally engaged in planning work and in handling extraordinary difficulties. Those of us who have seen these principles and methods in operation can testify to the correctness of these statements. We have seen men working no harder than before, but having been taught proper methods, accomplish results which make bonuses of 50 per cent on the former wages profitable for the owners. We have seen workmen, who without a trade have come to be considered and to consider themselves skilled workmen in a class as high as the trained mechanic. We have seen the removal of room mechanics, foremen, and even superintendents, when the workmen could no longer afford to permit the mistakes and neglects of these superiors to pass without protest.

Dr. Rudolf Roesler. My old motto, "Courage to the last," is almost the only excuse I can offer for taking a few minutes of your time, especially since my knowledge of your language is as yet unfortunately very poor. Without taking part in the discussion I would like to speak of some general facts which I think will have your interest.

2 In Europe, and especially in Germany, the greatest interest exists in the new ideas of economical organization and management in workshops, among which not least interesting are those of Mr. Fred W. Taylor. I think one important consideration is the belief that the new principles can help to weaken the *casus belli* for the struggle between capital and labor, to weaken the reasons for existence in continental Europe of Social Democrats and in America of Labor Unions, and to unite the workman and his employer by making their interests common. The following facts will illustrate this general interest:

3 The greatest German engineering society, the Verein deutscher Ingenieure, has resolved by almost unanimous vote of its branch societies to give a place to such questions in the columns of its well-known journal, Zeitschrift des Vereins deutscher Ingenieure; and a number of the most illustrious practical scientists and scientific practical men have promised to support this movement.

4 The tool machine works of Mr. Ludwig Loewe in Berlin is considered one of the best organized and managed factories of this kind. As much as is compatible with the German conditions this concern has been the first to take up the ideas which Mr. Gantt has explained. The book concerning the organization of the Ludwig Loewe Company by Mr. Lilienthal has proved the general interest by its wide circulation.

5 I come now to the most important because the most far-reaching fact. At the Technische Hochschule of Berlin in Charlottenburg there is a chair of Organization and Management, founded, I think, in 1 04. These principles among others are taught. According to the rules formed by the Prussian government and the Technische Hochschule, graduates in Mechanical Engineering, and I think also in Civil and Electrical Engineering, must take examinations in those two subjects. Thus every year as many as 1000 engineers go into the world with a knowledge of these principles.

6 I use advisedly the word Hochschule because there does not exist in English a term exactly analogous to the Hochschule; the American High School does not at all correspond to our Hochschule, which is the institution of the highest technical education in Germany.

7 In the case of each of the above-mentioned agencies, namely in

the scientific press, in the advanced machine shops and in our technical universities the matter of training workmen is considered of very great importance. In the Ludwig Loewe shop instruction cards and personal instruction by an engineer, in short, the fundamental ideas which Mr. Gantt has explained, are in common use. Also in the lectures at the Hochschule referred to, these matters are treated again and again.

8 I myself am astonished to see how many of the difficulties which seem to oppose the introduction and the continued application of those principles are removed in practice and how this system, based entirely on theoretical principles, can be adapted to the actual conditions. He who knows the system only by theory is not well able to judge it. These facts I have myself experienced. I was also astonished to see how Mr. Gantt has introduced his method with success into works whose daily product varies in form and character and in works other than machine shops.

9 I am sorry that I cannot illustrate by figures my remarks about the interest which the new ideas have for the men in European industries. The time between the moment when I was advised of Mr. Gantt's reading and the reading itself was not enough to get these statistics. I would be glad to give them to you later and then I hope with words more conforming to English grammar and pronunciation.

Mr. T. F. Kelly. Mr. Gantt's paper seems to me far too mild in its statements. My experience with the system for the last two years has been that every man in the plant, whatever his authority, has a specific job, and will get into trouble if he lays down on it. Under this system every employee becomes also an inspector, and for fear of losing his bonus protests vigorously against accepting material on which he has to work, unless both the material and former work are up to the standard for quality. It takes only a few touches on his pocketbook to make Mr. Jones a first-class critic on Mr. Brown; in other words, 300 employees means 300 inspectors.

2 The machinery must also be in first-class condition for operators to make their bonus. The "good enough" machinist cannot live under this system, as he not only loses his own bonus, but causes the gang-boss and the operator to lose theirs. These all act like a tonic on our Mr. Machinist, to do a good, quick job, or he soon finds out he is not the man.

3 The introduction of the bonus system is followed by a new feeling of pride, and the threat to move workmen from machines on

bonus to machines not on bonus is more effective than the threat to lay off or discharge.

4 The operators in our factory formerly measured their work to the clock; they now measure it to the task, and former shirkers are now among the most zealous of our employees. For instance, one of our weavers "suffered," and made our work suffer, through his disposition to malaria, but the bonus did more for his malady than any amount of quinine. On looking into the causes of an incipient riot the other day I found that our malarial friend had been trying to "beat up" a fellow workman who was not giving supplies fast enough and thereby keeping him out of his bonus.

Mr. C. H. Buckley. Of the many valuable features of this paper that of a definite plan drawn up for the task to be performed especially appeals to me. It is a mathematical problem worked out on scientific principles, just as an engineer calculates the necessary weight of a fly-wheel before it is cast.

2 Mr. Gantt also believes in educating the workman, by setting a mark which must be reached before he can earn the higher rate of pay. Of course the workman doing piece work, depending on his own resources, might become very proficient; though under effective tutorship, the same efficiency will be possible in a much less time. It is an excellent plan to set a higher standard than the average man would set for himself; by proper encouragement and instruction he will usually reach it. The bonus system will bring forth the best efforts of workmen and foremen, and as a result, the maximum product of the plant.

Mr. H. K. Hathaway. Mr. Gantt has brought out most forcibly a feature of the Taylor System that has received but scanty treatment in the various papers heretofore written on the subject. All of us who have served our apprenticeship to the machine trade, can recollect distinctly how little instruction we received, and how most of our knowledge was gained through a process of trial and error. The instruction of the average apprentice is at best a haphazard thing. His foremen, even though they may have the welfare of their apprentices at heart, under the old system of management, are unable to give him anything like the attention necessary to make him an efficient workman in the shortest possible time. Usually the instruction he receives is largely from the workmen with whom he comes in contact and is good or bad, depending upon whether the workman whom

he asks for information is himself a good mechanic, and whether he is inclined to impart the knowledge he possesses.

2 The writer has distinct recollection of setting up a job on a machine when he was an apprentice, in what appeared to him a perfectly proper manner, only to find by its pulling loose under the pressure of the cut when starting, that it was not properly supported. Under the Taylor System, he would have received proper instructions as to just how the work should be set.

3 The writer believes it possible under the Taylor System to turn out a first-class mechanic in about one-half the time taken under the old system of apprenticeship; an opinion borne out by results in a machine shop operated under the Taylor System with which he is connected. In this shop a number of young men, who came to us without previous experience at the machine trade, within a year and a half reached a point where they were capable of turning out work of excellent quality on any of the machines in the shop, and doing it in the time set by the Planning Department.

4 One thing to which the writer hopes Mr. Gantt's paper may lead is the adoption by the trades schools of the methods advocated. Most trades schools pay very little attention, if any, to the time taken by their students for performing the various exercises or tasks forming their course. Too often they have not nearly enough instructors, and boys waste a great deal of time trying to figure out how to do the various jobs; furthermore they have no conception of the feeds and speeds and depth of cuts that should be used in doing work in machine tools.

5 An instruction card prepared for each piece of work, showing the manner in which it should be set in the machine and explaining the various steps of the operation in their proper sequence and the tools to be used, would not only make it easier for the instructor but would enable the student to learn at once the best method, instead of using a method of his own with no foundation in experience.

6 If proper instructions and tools were furnished, and the machine and belts kept in good working order, a definite time could be placed on the job, and the student made to acquire habits of industry. The present methods of instruction may be fine for developing any latent ingenuity of the student, but they certainly waste valuable time. It would be ridiculous to expect a child to write a composition without having first learned the alphabet.

7 Professor Agassiz once gave a student a fish and simply told him to go and study the fish and come back in the course of a week

and tell him what he had found out about it. Naturally enough, when the student came to him, Professor Agassiz told him that his observations were very superficial and sent him to spend another week in studying it. The student did eventually know something of the nature of the fish, but he took about three times as much time as he should have taken to acquire the knowledge.

8 The greatest value of the system of training outlined in Mr. Gantt's paper lies in the fact that in busy times, when skilled workmen are unavailable, it is possible to train inexperienced men, who are intelligent and ambitious, to turn out good work rapidly. The writer has seen an absolutely green man, trained under this system so that in less than a month he was capable of turning out work on a drill-press as satisfactorily as an old hand; of course during this time the "gang-boss," "speed-boss," and "inspector" were almost constantly with him helping and instructing him. After a workman has learned to run a drill-press successfully, he can be trained in about the same time to run a milling machine, lathe or planer.

9 One of the best examples of the efficiency of this system of training workmen, is the results achieved with young college students taken on after their Freshman or Junior year, in the shops with which the writer is connected. After their year in the shop they return to college and complete their course. One object of this plan is to train them in habits of industry, and this object is most successfully accomplished.

10 During this year they are governed by exactly the same regulations as other workmen and are allowed no special privileges of any sort. Under the instruction of the various functional foremen they do effective work from the start. They are started on work of a very simple nature, such as running a sensitive drill-press or cutting-off machine, from which they progress to a radial drill doing a more difficult class of work, thence in turn to the turret lathes, milling machines, planers and engine lathes, spending about two months on each machine; almost from the start these students accomplish the tasks set and earn their bonus, and before the expiration of their time on each machine can turn out as much and as good work as old experienced hands.

11 The progress that can be made with adequate instruction is astonishing even to one familiar with this system, and the writer sincerely hopes before long to see the system applied to the trades schools and the college shops.

12 That the Taylor System is a system whose success is due to

teaching and helping the workman, should be brought out more prominently. In the first place, the proper tools, in first class condition, are provided, and his machine and belts are kept in good repair. Secondly, the gang-boss must show him how to set his work up quickly and in the best way, and not only tell him, but demonstrate it. The inspector must not only detect defects in his work, but must explain, when the workman starts on a job, the drawings, the degree of accuracy, and the kind of finish required. The speed-boss instructs him in the actual operation of his machine, and the setting of his tools, feeds, speeds and depth of cuts, and is prepared to help him if necessary by actual demonstration.

13 Under this system a workman can turn out from two to four times as much work, as his efforts are not largely consumed in finding out what he is to do, devising ways to do it and struggling against discouraging adverse conditions over which he has no control.

Mr. Charles Piez. The bonus system of rewarding labor, which Mr. Gantt describes in his paper, can hardly in itself be considered a system of instruction, and is, in fact, no more an instrument to this end than any of the well known schemes of compensating workmen. It is through the methods Mr. Gantt employs that his work becomes a most effective means for training workmen in habits of industry and coöperation.

2 What appeals to me most in Mr. Gantt's presentation is its distinctly human tone; the spirit of helpfulness toward the worker which it evinces. He recognizes that people as a rule are willing to work at any "reasonable speed and in any reasonable manner if sufficient inducement is offered for so doing, and if they are so trained as to be able to earn the reward," and he finds in the application of his system that "an *instructor*, a *task*, and a *bonus*," prove most useful.

3 Satisfying a man's desire for acquiring skill, or proficiency, setting a task that is reasonable and well within his capacity when properly trained, and paying him a suitable reward beyond his day's pay for accomplishing the task set, constitute a most complete and comprehensive system of training for modern specialized production.

4 Mr. Gantt recognizes the fact that reörganization often means only a change of mental attitude, and that it can, therefore, be best accomplished by persuasion and example. Then, too, while estabishing fixed methods of performing tasks, he allows ample opportunity for initative on the part of the worker; in fact, he stimulates and directs it.

5 In these days when systematizing of industrial establishments has become a recognized specialty in the mechanical world, a few thoughts suggested by Mr. Gantt's paper may not be amiss.

6 There is abroad today a great deal of what might be termed System Idolatry, which manifests itself in the belief that system produces output, when, as a matter of fact, it simply indicates the lines along which maximum output can be attained; and because of this erroneous conception the system assumes the rigidity of a creed, and the various printed forms of which it makes use are invested with a sanctity that is intended to place them beyond the reach of suggestion or criticism whereas they are frequently modified without any departure being made from the underlying principles.

7 The adaptability of an already existing organization, from which the material for carrying out a system must be drawn, the peculiarities of the product, and the demands of the customer must be given full consideration. If the System is considered the important thing, and organization, product and customer must bend to its lines, is it any wonder that attempts at systematizing a plant may fail to result in the full economies promised? And they fail, not because the system is inherently wrong, but because of the fanaticism of the enthusiast applying it. Tact and good judgment must be supplied by the introducer or receiver of the system.

8 I am a firm believer in the efficacy of shop system, for in its essence it implies the production of work along lines of least resistance and greatest economy. But direct lines are not always the lines of least resistance, particularly when they run counter to peculiarities of ability or temperament in an otherwise efficient organization. It seems unnecessary to compel an organization to conform to a system chart, because it is much simpler and more effective to make the chart conform to the abilities of the individuals composing the organization.

9 The first step, even in the mildest form of reörganization, is a partial disruption of the existing organization, and great care and tact must be exercised, lest in the rebuilding, discontent and discord creep in. The line between profit and loss in most establishments is so fine that even a single element of discord can destroy that intangible, profit-making quality, known as team spirit. It is on this account that my interest lies, not so much in this system or that, as in the personality and methods of the men applying it.

Mr. C. N. Lauer Mr. Gantt has adopted a humane, as well as a

scientific, basis for his system of management, and this will do more than any other element towards broadening the field of men working on the problems of management and organization. Mr. Gantt has provided for all the essentials of good management, namely, standardization, task-setting, piece-rating, etc.

2 It has been the writer's experience that the best results, after a definite plan has been laid out, are obtained if the spirit of coöperation can be engendered in the workmen. The manager who depends entirely upon his own ability to drive his employees is bound to fail by just so much as the employee holds in reserve against contingencies which he feels may arise through the whim of the manager. It is the spirit of helpfulness which runs all through Mr. Gantt's paper that especially prompts the writer to pronounce it well worthy of serious consideration.

Mr. Lewis Sanders. I am convinced that the method of shop management described by Mr. Gantt is the logical and correct one for getting the maximum production from our factories at the minimum expense. There is one point that should not be lost sight of, and that is that this system is not a substitute for the proper training of apprentices, and in no way decreases the desirability of it, and I do not think that Mr. Gantt advocates it as such. It is a system that should insure the maximum output from both the skilled and the unskilled.

2 The great advantage of having well-trained men under this system will be that they will continually be improving the manufacturing processes, so as to cut the time of work, and that these improvements will then be applied to the work of the untrained. On the other hand the accurate analysis of the method of doing a piece of work, required by this system, should be added to the course of training of the apprentice. We can then very much increase the speed of special work, where only one or two pieces are made, when put in the hands of men so trained that by practice the elimination of unnecessary operations will become almost instinctive.

3 I have not had the opportunity for direct observation of any plant where these methods have been introduced, but I have seen individual cases where a little study of the methods of doing a piece of work has resulted in marked reduction in time, and this often in classes of work where saving would not be expected; for instance in reducing the time necessary to read thermometers. I recall a test where one of the observers had to read twelve thermometers once

every two minutes. He never succeeded in reading more than eight, and was on the go continually; I took his records and tried to see what could be done; the first few readings I got no more than he had, but by studying just where to stand so as to get the light unobstructed and not to be obliged to shift my position, in a short time I was able to read all the thermometers in 1 min. 50 sec. In another case where three men were required to take the observations, I had to make some laboratory tests with only one available to assist me, and by a little study of the method of taking the readings we soon found that two could take them quite as easily as three.

4 At Schenectady I have seen a 2000-kw. vertical turbine used for experimental work, completely dismantled and a new set of wheels put in, and the machine re-erected and running within 24 hours from the time steam had been shut off. This was done by two machinists and a cranesman. I am informed that the men have now become so expert that they do it in twelve hours.

5 A factory in which I am interested bid on turning out a certain small piece of work in quantity. We made a detail study of every fractional operation involved and made our bid on our estimates, being unusually careful in our figures because the shop had never done any work of the character. Our bid was so low that it was returned to us with a request to revise it, as the customer was sure it was less than the labor and material cost. The customer had already made 80,000 pieces. We had sufficient confidence in our figures to insist on their acceptance as they stood. Our estimated labor and material cost was 36 cents, and at the start they cost us 38 cents, which was cut to 34 cents when everything was being done as laid out. The customer was never convinced that we did not lose money although we made 44 per cent profit. But the customer had never been compelled to analyze every single step in turning out the goods, to find out what the cost should be. They had merely made some and knew what they did cost. We should probably have done the same if we had not been compelled to make the analysis. One of the operations on this job was the setting of the piece in a jig, the man who was put on it taking regularly 1 min. 40 sec. After a study of the exact motions required to pick the piece up and set it accurately we showed the same man how to do it in 20 sec.

6 In speeding up a shop a distinction should be sharply drawn between work done at high speed and work done in a hurry; the first will give perfect goods because the speed is attained by elimination of all the unnecessary motions, the latter bad work because it

is a speeding up of all the operations, necessary and unnecessary. High speed work also involves less labor on the part of the employee, than slow speed work. The man who could read only eight thermometers was going all the time; but there was 10 sec. to rest when reading twelve. The machinist who set the piece in the jig in 20 sec. was doing less work than when he spent 1 min. 40 sec. at it, and the same with the machinists erecting the turbine.

7 This method of handling men is in my opinion susceptible of wider application than machine shop practice. It should give good results wherever the same class of work has to be done repeatedly. The chief engineer of the St. Regis Hotel, Mr. Jurgensen, has had a somewhat similar plan in operation in their power plant for several years, and it has resulted in considerable economy of operation. Those interested in power plant management will find it worth their while to investigate what he has accomplished.

8 There is no question but that it requires a considerable knowledge of men to introduce successfully such a system as Mr. Gantt advocates; the theory might be followed out exactly, and the whole system yet be wrecked by lack of common sense and a little knowledge of human nature. After it has become the routine of a shop, however, that shop should be a very easy one to manage, providing good faith is kept with the men. One obstacle to securing high-speed work is the suspicion of the men that rates will be cut if they show that they are able to earn more than a certain sum per week; and they have been given ample grounds for this. On this account the time determined by expert analysis is the only reliable standard as the men will go as slow as they dare.

9 While quite true that there is a maximum wage that we can afford to pay, it is also true that an agreement with a workman should be lived up to as strictly as a contract with a customer. We have to fulfil our contracts even if we have made an error and find them unprofitable; the same should be done with the workman when we find that we have set too easy a task. Tasks set should therefore hold for a definite period, say a year, unless a change in method is made. Before the task is set it should be the subject of accurate investigation to determine the minimum time possible. If the task set proves too severe it must be corrected at once, as the workman is not a party to the setting of the tasks.

Mr. J. C. Jurgensen. Mr. Gantt's methods are based on a concrete knowledge of the human element, which is sure to result in fair dealing to both men and employers.

2 I feel justified in speaking on this subject, since for more than five years I have used a sort of apprentice system for producing reliable and efficient help for the operation of power plants. Although the conditions in an engine room differ very widely from those in a shop or factory, yet the same principle holds, that men who can make good must be trained. When this is once fully realized, every shop and plant owner will look more favorably at the idea of maintaining a training course for his men, as a part of his routine business.

3 To succeed in the training of men, the leader needs to be well-equipped—he must be able to control himself in the handling of his men, he must be thoroughly familiar with the work and the duties of all his men, and he must be a close student of human nature. He ought to have the instincts of a teacher, he must be a good adviser in many troubles, and above all, he must be just as both judge and jury in settling disputes; he must be sympathetic and yet firm and have determination enough to see that orders are carried out to the letter.

4 The leader must make his men understand that it is one of two things: advance, or make room for a better man; and inducements for compelling a man to see it in that light, such as a nine-hour work-day, reasonable wages, provision for advancement, etc., must be present. It is a question of setting right both the employer's mind and the workman's job, and the Golden Rule must be applied before success is possible. As a general rule, it is necessary to hold out material inducements for acquiring better skill and efficiency. With some, but not with many, ambition and the right temperament will bring this about.

5 To secure safety and economy in an engine room, the men must become willing workers and must take pride in the engineer's department. To reach this point, a man must feel that his position is secure, with a good record, and that advancement and benefit are certain to follow; on the other hand, that a continued bad record will bring discharge. Advancement according to seniority is all right with a qualifying clause—instead of giving preference to the oldest man in the service, make it the *oldest best man.*

6 If opportunity for material advancement is not present when a man has gained his apprenticeship or has reached the highest station possible, we find a new job for him. This is not so hard as it would seem, since other plants are generally willing to take him, and this provides a further chance for advancing the members of the department. Success along this line cannot be had without coöperation,

and this we obtain mainly through a system of rules, and an organized school of instruction in which the Chief Engineer is the Chief Instructor, and each man in charge of a section, an Assistant Instructor in that section of work.

7 In the fire room, the men are paid a good salary for producing one boiler horse power for a certain amount of coal containing a certain per cent of ash: 10 per cent of the value saved over this standard goes to the fireman as a cash bonus each month; on the other hand whatever is lost, according to the standard, is deducted from that man's monthly saving. The head fireman receives a sum equal to half the bonus of each fireman—he is therefore vitally interested in having each man do as well as possible. If a fireman cannot make a bonus, his job is not secure.

8 The coal passers also receive, divided equally between the three shifts, a sum equal to half the bonus of each fireman—they are therefore much interested to see that all the firemen "make good," and yet there is no motive for collusion between the men. It is soon shown that the best policy for both the company and the men is a continued effort to make the bonus as large as possible. All the men in the department are thus taught to follow both daily and monthly standards in work and expenses.

9 To illustrate the scope of our Engineering Department Training School which is a part of the Men's Relief and Educational Society I will mention the objects of the Society, as stated in our by-laws:

Section 1 The objects of this Society shall be the raising of funds to provide a weekly relief income to members in good standing during illness or accident and such other relief as may be deemed advisable, and to assist in defraying burial expenses of a deceased member; also, to defray the expenses incurred in carrying on the training course which constitutes the Educational Branch of this Society.

Section 2 As a further relief, members who are in good standing may apply to the Society for loans, not to exceed 10 days pay of such members' monthly salary. Loan to be paid back in four successive and equal monthly installments, plus 1 cent per dollar per month, on amount due to the Society.

Section 3 The object of the Educational course is to give such practical instruction and example as will further a spirit of manhood and induce the members of the Department to become self-reliant, observing and manly men. Training such men to become safe and conscientious workmen, worthy to receive the Company's Certificate of Merit for two years service.

Section 4 To ambitious holders of the Certificate of Merit, the training course will endeavor to supply the technical information most needed to make such workmen qualify as safe and efficient Operating Steam Engineers, worthy to receive the Company's Operating Steam Engineers' Apprenticeship Certificate for five years service.

Mr. Willis E. Hall. Mr. Gantt's paper dwells upon a component in our industrial situation that must soon have more consideration than it has received in the past. For comparison and analysis, and to avoid confliction, his system should be divided as follows:

a The task basis of compensation, those failing to reach a fixed degree of efficiency receiving only their established day rate.

b The use of an instructor. The duties of the instructor are (1) intelligent determination of the time, on a fair basis of compensation for the work that is to be done; (2) instructing the men to do the work in the way on which the price was computed, which being based on the latest information must therefore be the best method of accomplishing it.

2 Why does the task system of itself promise any better results than those obtained under the piece-work system? The term piece work is here intended in its broadest sense, to include all methods of compensation based on the amount of work done. It therefore includes such modifications designated as the bonus, premium, differential, profit-sharing, gain-sharing, etc. On the contrary the task system seems to eliminate day work only partially. For instance if the employee can not do the work within the time set for the task he receives no additional compensation and his day rate prevails. Assume, then, that the delinquent is replaced by one who can meet the task (and the task system has no monopoly of procedure in this respect, as the same degree of vigilance should be exercised with any system), until the plant has, finally, the very best set of men obtainable; and further that the men are instructed to the highest degree of efficiency (and here again the task method does not possess exclusive privileges). It is unnecessary to substantiate the statement that there will still be a vast difference between the most and the least efficient of the force.

3 How then will you set the task? If it is set so that but a limited number, say 65 per cent can meet it, then the remaining 35 per cent must be eliminated from the rewards. The answer may be that this remaining 35 per cent will have the day rate adjusted according to their relative efficiency. To that extent the task is not better than the day-rate system. The other alternative is to set a task that the least efficient can meet, but neither is this fair to the employer or would it promote efficiency in the employee. In short,

with the task system proposed men must be practically equal in efficiency or it is only a partial eliminator of day work. Unavoidable ruling conditions due to the unequal efficiency of men, instructed or otherwise, seem to prevent this from being other than a partial day-work system for producers. However, this is of minor importance in comparison with the other feature in Mr. Gantt's paper, the use of an instructor.

4 For greater productive efficiency, to the mutual benefit of employer and employee, the use of an instructor, combined with the intelligent piece-work price-setting described by Mr. Taylor (Vol. 14 of Transactions), seems full of promise. That the use of an instructor establishes "habits of industry" of which Mr. Gantt speaks, must not be misconstrued. There is quite a difference, with apology for the terms, between the "sweating process" and "farming the work;" both extremes are equally demoralizing. It is the medium position that is always most difficult to assume at first and the one that is most desirable. It is only by accident it is reached by the haphazard method of establishing piece-work rates at present so generally used, which usually results in one or the other of the two extremes, until the price, after many adjustments, is about where it should have been originally. But by this time some change in the method of doing the work makes necessary a new start. Instruction, establishing industry, does not mean the "killing pace." Ordinarily it is the contrary. Commotion is not necessarily industry; usually they vary in inverse ratio. There is one best way of accomplishing work and that way, when once learned, will be the easiest. Intelligent setting of piece-work rates or task limits with an instructor eliminate the more flagrant abuses of the system. Some existing variables cannot be met with the present status of the mechanical arts, but they are insignificant in comparison with the evils eliminated, and our range of vision should not be limited by a disadvantage of perhaps 5 per cent when there is a 95 per cent gain in sight.

5 Most of us that have been identified with piece work have, no doubt, felt for some time that a modification of the present atrocious abuses of the system was necessary. Mr. Gantt would render an invaluable service to the Society by giving it a comparison of results obtained, with the use of an instructor as the only variable factor. For instance, where the Taylor differential system is in vogue, what additional advantage is obtained by the full use and authority of an instructor? The additional cost should not be overlooked.

6 One other feature, though this is somewhat of a digression

from Mr. Gantt's paper, should be made a part of the duties of the instructing and piece-setting department. It is that of time-checking, where either the piece-work or the task system is in vogue. For instance a producer spends actually 7½ hours on piece-work but has been in the shop some 10 hours in all. There is a loss "for good and all" of 2½ hours of productive time. In other words, what percentage of the producer's time is lost in this way, and why? Is it not safe to say that most of the establishments using piece-work ignore this point? In one instance where the tonnage of a plant was increased more than 90 per cent over what was formerly, at least, normal tonnage, about 30 per cent of this increase was due to the elimination of the annoying and demoralizing delay known as "waiting for work." And yet this delay was previously not over-conspicuous and its magnitude was not appreciated until it was eliminated. There was practically no rate-cutting.

7 Nothing about a shop can be more easily hidden, either intentionally or unintentionally, than this loss of time. The greatest incentive to indulgence in it is a slip-shod method of rate-setting, especially when the known practice of the employer is to await the first opening to slash the price. For this the employer is responsible. His loss is dependent upon the nature of the product and the relative proportion of organization expenses to producer time. The loss to employee is easily computed. That it can be more satisfactorily checked and controlled by intelligence than by the present method of price-setting is self-evident. This, with the elimination of favoritism, should answer the argument so often made, in defense of the present method of setting piece-work prices, that it does not make much difference so long as the "average" rate is about right.

Mr. Harrington Emerson. There has been an almost unanimous chorus of agreement with Mr. Gantt, and music sometimes sounds better if there is now and then a note of discord. I once asked Mr. Gantt what happened in case one of the workmen whose time he had set managed to do the task in less than the time prescribed. Mr. Gantt very cheerfully answered that in that case the workman would take his place and he would have to take the workman's place. Since then my assistants and myself have had to standardize over 100,000 different jobs, and we have never been able to realize the accuracy which Mr. Gantt so flippantly claimed. The reason probably is that Mr. Gantt's work lay along standard lines, working on standard material, in standard manner, while my work lay in repair shops,

working on unstandardized material, unstandardized jobs and unstandardized conditions. In such matters as locomotive repairs we were able to predetermine, within four per cent, both time and cost of the aggregate, but not to strike the exact time of the jobs that entered into repairs.

2 Now Mr. Gantt comes forward with this paper, and we have a chorus of assent to the statement that by training the workmen the problem is solved. My experience has been that this is not at all the end of it. It is easy to train the workman in habits of industry and coöperation, but when you have provided a method for training him, you have not touched the real problem. What I would like from Mr. Gantt is a paper on training managers in habits of logical thought and coöperation.

3 To illustrate the enormous practical and economical importance of this question, I quote the figures on locomotive repairs per mile on four out of the five trunk lines running west from New York. This is not a very accurate standard, but nevertheless it is used. On one of these, 44 mills maintains the power in first-class operating condition; on the second road, the cost is only 7 cents; on the third road 12 cents, and on the fourth road over 16 cents. This fourth road has a mileage of 30,000,000 miles, and the cost is over 12 cents more than on the first road, making the amount of money lost per year $3,600,000. Yet this road has good grades out of New York, while the road that shows the lowest cost has the heaviest grades. (This last statement was in answer to a question from Mr. A. H. Emery.—EDITOR.)

4 The trouble does not lie with the workmen. No doubt the workmen are wasteful, no doubt they have not been fully trained, but the trouble lies absolutely in the managements which have not yet awakened to their problems, and accept enormous wastes as inevitable.

MR. MILTON P. HIGGINS. When this marvelous proposition was first advanced two years ago by Mr. Taylor, I looked upon it as a departure of very great promise, and I have not been disappointed in its results. In addition to considerations of shop discipline, efficiency, output, profits, and the well-being of the workmen, as a means of education, I believe it is equally important and efficient.

2 In place of the old system of apprenticeship we have now the training school, the apprentice school, the trade school; but the shop which trains its apprentices to the best advantage to meet modern

requirements has within it a trade school. That is, the task, intelligent analysis of methods, and the bonus, are most effective elements of disciplinary education, not only important as developing skill and efficiency, but developing intellectual capacity and manly character. I believe that the training given in the shop through these methods may be as good as any line of mental and intellectual discipline, that can be offered in a university.

3 In regard to the suggestion Mr. Emerson made, of training managers and superintendents, if we wisely and faithfully train the skilled workmen, are we not training the managers? If we want large trees in the forests, had we not better give our attention to the nursery?

Prof. Wm. Kent. At the meeting in Detroit in 1895, when Mr. Taylor made an address upon a similar subject to this, he met with unanimous dissent from the older men; and I got up in that meeting and said that a man over fifty years of age could not be expected to appreciate Mr. Taylor's paper, and that the revolution in the industry that was to follow from Mr. Taylor's plan was to come from the work of such younger men as Mr. Gantt. I am glad to see that Mr. Gantt has verified my expectations.

2 The hopeful thing about this paper, which I regard as the most important that has ever appeared in the Transactions of the Society, is that it is in harmony with humanitarian ideas. I am glad to see this sentiment throughout the Society. There would be more of it if more of our mechanical engineers would go into shops, instead of into power plants and drafting offices, and offices downtown.

3 The trouble with the managers has been mental inertia. Men of great brain power, enterprising, and in important positions, will not take half an hour, which is worth perhaps $50 to them, to think on a problem which might end in saving them $10,000 a year in the shop. That kind of manager, however, is rapidly being displaced and the younger men, who are willing to do some thinking along the lines of Mr. Gantt's paper, are coming forward to take their places.

Mr. James M. Dodge. I have been asked whether the concern that I am connected with has abandoned the Taylor System, and I desire to be recorded as saying that we have not abandoned it; on the contrary, it is working to our complete satisfaction in Philadelphia and Chicago, and we are introducing it as well in our Indianapolis plant. Mr. Taylor told us five years ago that the introduction of

his system would be of great saving to us in normal times, but that its greatest value would be shown during periods of commercial depression, and I am very glad to be able to say that Mr. Taylor was absolutely correct in his statement. Our business during the past year (1908) would have shown a deficit had we been working under the methods we thought quite excellent prior to our adoption of the Taylor System, which carried us through this twelve months during which our business was curtailed about one-half, with a moderate profit showing on the right side of our annual statement.

(Question by Mr. Frank A. Haughton as to the difficulty in holding together their organization of experts through this period of depression.)

2 I am glad that question was asked, because we did not experience the difficulty suggested. Under the Taylor System the functions of the different members of our organization are so clearly defined, that we were able very promptly to reduce our expenses by demoting a number of our workers, thus avoiding the demoralization of the sub-divisions of our organization. We took the men into our confidence and explained to them the exact situation, and they cheerfully accepted it; in fact we had no trouble at all. Our organization was and is intact, and, as Mr. Taylor told us, can be readily expanded to any desired extent as soon as increased orders make it necessary.

The Author. A system of management may be defined as a means of causing men to coöperate with each other for a common end. If this coöperation is maintained by force, the system is in a state of unstable equilibrium, and will go to pieces if the strong hand is removed. Coöperation in which the bond is mutual interest in the success of work done by intelligent and honest methods produces a state of equilibrium which is stable and needs no outside support.

2 Until within a few years the mechanic was necessarily the source and conserver of industrial knowledge, and on him rested, therefore, the responsibility for training workmen. With the advent of the scientifically educated engineer, capable of substituting a scientific solution of problems for the empirical solution of the mechanic, the responsibility of training workers naturally shifts to his shoulders. If he accepts this responsibility, and bases training on the results of scientific investigation, the efficiency of the workman can be so greatly increased that the manufacturer can afford to give those that take advantage of this training such compensation as will secure their hearty and continuous coöperation, thus making the first permanent advance toward the solution of the labor problem.

3 This was first demonstrated by Mr. Fred W. Taylor, and the whole industrial community has already been profoundly influenced by his work.

4 It was well said yesterday that the work of the engineer has been less appreciated than that of any other learned profession, and a broader recognition of his work will have a marked influence on our civilization.

5 He is carrying forward under the direction of science the work that was begun by the mechanic who first learned to chip flint or make a fire, and it is he alone that can lead the mechanic of today to a better understanding of his problems, and the capitalist to a better appreciation of their solution.

THE PRESENT STATUS OF MILITARY AËRONAUTICS

By Geo. O. Squier, Ph.D., Published in The Journal for December

ABSTRACT OF PAPER

The scope of this paper will best be shown by the following Table of Contents:

Successful Military Dirigible Ballons: France, The Patrie and Ville de Paris; England, Military Dirigible No. 1; Germany, The Gross, the Parseval, and the Zeppelin; United States, Dirigible No. 1. Balloon Plant at Fort Omaha, Nebraska.

Some General Considerations which Govern the Design of a Dirigible Balloon: Buoyancy and Shape; Resistance of the Air to the Motion of a Projectile; Analogy to Airship; Aërodynamic Adjustments.

Representative Aëroplanes of Various Types: The Wright Brothers' Aëroplane; The Herring Aëroplane; The Farman Aëroplane; The Beriot Aëroplane; The "June Bug."

Some General Considerations which Govern the Design of an Aëroplane: Support; Principle of Reefing in Aviation; Determination of k for Arched Surfaces; Resistance and Propulsion; Most Advantageous Speed and Angle of Flight; Stability and Control.

Some General Relations Between Ships in Air and in Water: Helmholtz's Theorem; Skin-Friction; Relative Dynamic and Buoyant Support; Motors; Propellers; Limitations.

Hague Peace Conference; Influence on Military Art; Delimitation of Frontiers; Interior Harbors.

United States Signal Corps Specifications for Heavier-than-Air Flying Machine.

United States Signal Corps Specifications for Dirigible Balloon.

Bibliography.

DISCUSSION

Dr. W. J. Humphreys. The point I am to discuss primarily is the connection of the winds with flying machines. It will be very important for practical navigators of the air to have maps of the winds. Such maps are issued certainly once a day, and may be had

twice a day, or even more frequently, if there be a call for them, as I fancy there will soon be on account of the development of the flying machine. That is evident to every one, but there are one or two points with which perhaps you are not quite so familiar.

2 We find that there is often a great difference in the direction of the winds as we ascend. I have seen the wind change in direction by as much as 30, 45 or even 90 deg., at an elevation of not more than 1000 ft., and in one or two instances I have seen winds differ in direction by as much as 180 deg., at a distance of 1000 ft. above the surface of the earth. It will be necessary to take account of these facts in the navigation of the atmosphere.

3 The winds change greatly in velocity also, at short distances above the surface of the earth. We find by experiment with kites and balloons that near the surface of the earth the air is exceedingly turbulent, after the order of a choppy sea, but at distances of 500 ft. or over, it is more like regular billows, while at an elevation of 1000 ft. the wind is practically steady. Here the aëronaut would neither be in reach of the billows nor run into the ruts and hills and bumps of the invisible air.

4 The direction of the wind can be determined frequently by simply observing the clouds. That will be an aid for the practical navigator, but these changes are different at one place from what they are at another place, and that the practical navigator himself will have to watch. Maps cannot be made for this particular purpose, but what we can get from the map primarily is where the storms are and from what direction the winds will be likely to come for ten or fifteen hours. We have found that around the center of the cyclonic storm, winds will blow in the northern hemisphere counter-clockwise, so knowing the position he is in with reference to the center of the storm, the observer can determine the direction in which the winds will blow. In this section of the country the winds which run at all high in summer always blow practically parallel to the coast line from New York to Boston, while in winter the upper winds are more nearly directed towards the ocean. That may be of importance for the practical navigator to take into consideration.

5 There are many more points of this type that I might bring out, but I believe these are really the salient ones.

Prof. John A. Brashear, who was intimately associated with Professor Langley for three years, in his scientific researches, wished to emphasize the recognition given his work by Major Squier, and thus

to pay a debt of gratitude to a man whom he considered one of the greatest physicists, as well as one of the most generous men of this country.

2 Par. 159, on the most advantageous speed and angle of flight, had recalled to him the very large number of experiments made by Professor Langley to determine these two important points. Professor Langley invented a machine, which he called a dynanometer-chronograph, to record all the experiments which he made, and to his honor be it said that he started into the work not for the purpose of building a flying machine, but of determining the law of flight, and he repeated his experiments time and time again and sent nothing into the world of which he was not practically certain; just as in sending out his experiments on the absorption of the atmosphere as a factor in the possibility of life on the earth, he was just as particular with the data in regard to the former as with reference to the latter.

3 Professor Brashear referred to Professor Langley's failure in attempting the last flight with his aëroplane, and to the unfortunate publication of unjust reports which did so much to check his career and wreck his life. He said: "I want to add one word more, and that is the sad side of it. We have heard of Fulton's being stoned when he started his steamboat and of the anathemas hurled at the men who have been pioneers in the advancement of the world's knowledge. I went into the Smithsonian Institution just after Mr. Langley's unsuccessful flight on the Potomac. Mr. Langley heard my voice in the outer office and came in to me, taking my hand, with bowed head. 'Mr. Brazier,' as he always called me, 'Come in, I want to talk to you.' Picking up two little pieces of steel broken from an original piece and handing it to me he said, 'Here is what has wrecked my life. My life-work is a failure, this broke, and turned my ship into the Potomac instead of up into the air.' And that man could not be aroused from his lethargy and depression. I said to him, 'Professor Langley, your work in the study of the earth's atmosphere and the possibility of life is enough for one man to do;' but it was no consolation to him.

4 "Let us not forget to say a few kindly words, even in the failure of men, and to remember that a great part of this life and of our success in it is made up of our seeming failures."

Mr. Geo. L. Fowler, who was also associated with Professor Langley, gave a few reminiscences of his work, outlining it as follows:

2 Professor Langley started with the idea of determining the resistance and lifting-power of various types of aëroplanes. He

started with the simplest of little instruments, which he projected from the balcony of the Smithsonian Institution down into the larger room beneath. These experiments developed first a motor-driven machine, for which he used the contraction of a rubber band, and then supplemented the work which I believe he had already begun at the Allegheny Observatory, by establishing a whirling table in the lower part of the Smithsonian Institution by which he measured the resistance and lifting-power of the wings of various types of birds and almost every conceivable shape and form of aëroplane, and also obtained the propelling-power of various types of propellers fit for use in the air. On one occasion he built a large propeller mounted on a hand-car which he used on the Pennsylvania Railroad, and obtained possession of a small track on which he drove that car for several days in order to obtain the resistance of the car and also the propelling-power of the machine.

3 I think it was only after the first successful flight of his motor-driven aëroplane that Professor Langley realized the importance of balance. Any one who has seen Orville Wright drive his machine at Fort Myer must recognize after witnessing it for even a few minutes the absolute control which he seems to possess over the machine, and if any here had been present at one of the flights, and had been invited to take a ride with Mr. Wright, they would have gone with perfect confidence. The machine started down a slight incline or monorail, rose, and apparently gave a slight dip—it looked to me as though he had his upper wings elevated to give him the rising power, found himself rising too suddenly, turned back and after a slight hesitation flew away. The steering of the machine was under absolutely perfect control. One thing about the Wright machine I think Professor Langley overlooked in his early experiments. In all early work and in the publications of the Smithsonian Institution, the principle of the bird soaring on motionless wings was repeated again and again. Now an eagle soaring high above the earth appears to be on motionless wings; but, if you will watch a buzzard rising over the land, or stand at the stern of an ocean steamer and watch the gulls following it, you will see that they do identically the same thing as a boy walking on a railroad track or a tight rope walker on the rope with a balancing pole—the soaring bird with his wings out is constantly making sudden, quick motions. That point, which Professor Langley failed to overcome, the Wright Brothers overcame by a slight warp in the upper wing, which has been one of the causes of the success which they have obtained.

4 Professor Langley found on his first experiment that balancing was a great necessity. In his first machine, the weights were carefully calculated in order to determine the exact specific gravity of the whole thing, and then a small hollow tin can four or five inches in diameter and six or seven inches long was placed on the bowsprit to lower the specific gravity to such an extent that the whole machine would float if dropped into the water. When first cast off, the can was pretty well forward, and the machine promptly made a dive head-foremost down into the Potomac. Next the can was moved back four or five inches, throwing the center of gravity back so much further that the machine went up into the air and dropped back tail-foremost into the water. Then the can was again shifted to a point about halfway between the two, and when cast off the third time it swung around in a circle and flew off for a distance of three-quarters of a mile. That was the first successful flight, and the beginning of the application of balance, the self-limiting balance.

5 In his first experiments Professor Langley worked along the lines of least resistance and lowest weight. Everything about the aëroplane was of the lightest possible type; what he wanted was to fly, and the consideration of stress of material would come later; as a matter of fact in calculating his stresses he ran them up almost to the breaking-point of the material, so that there was practically no factor of safety, and perhaps this was one of the causes of his later failure.

6 I remember once a gentleman saying to him, in discussing his boiler, "Well, Professor Langley, that boiler will make steam, but it will be very uneconomical in the use of fuel." "Never mind that, we will burn gold under the boiler, if necessary. What we want is steam," was the reply. The result on the first successful machine was a very great steam producer, but at the same time a very uneconomical boiler in the use of fuel. His engine was of the simplest possible type, a little engine with a cylinder of about 33 mm. diameter, 73 mm. in stroke. Everything about the work was done on the metric system. He succeeded in developing about one horse power. The cylinder of the engine was a small steel tube, about one millimeter thick, and as the steel would not make a good bearing surface for the piston to move backwards and forwards in, he bushed it with a cast iron bushing of the same thickness, which gives some idea of the delicacy of his work. The pump that he used to force circulation in the boiler was of the same small and delicate type. The only thing about the machine really efficient from the standpoint of the steam engineer was his burner. He realized that he must burn his fuel to get as high a

temperature as possible, and spent a great deal of time in developing his burner, which was exceedingly efficient, and with that small machine of about 14 ft. span of wings, he succeeded in making a successful preliminary flight and the machine did repeat this flight on several occasions afterwards.

7 That is a brief outline of what he attempted to do and what he really succeeded in doing, but beyond that, the science of air dynamics has probably been worked in more thoroughly by Prof. Langley than by any other man, and it is doubtful if any one will ever devote so much time and energy to the solution of a scientific problem that promises so little in the way of practical results, as did Professor Langley when he undertook to find out the true theory of flight.

SALT MANUFACTURE

By Geo. B. Willcox, Published in The Journal for Mid-October

ABSTRACT OF PAPER

Some details of construction are explained, that have proved successful in plants for the manufacture of salt by the "grainer" process, including the design of evaporating grainers of reënforced concrete, automatic devices for removing salt from the grainers as fast as it is formed, details of a conveyor for handling salt, and a device for loading salt barrels into box cars.

The purpose of the paper is to draw attention to a few of the features of grainer salt production that may be of general interest to the mechanical engineer in lines other than salt manufacture.

DISCUSSION

Mr. C. F. Hutchings. Before lumber became scarce, the "grainer block" was an adjunct to practically every saw-mill in Michigan. By using their exhaust steam in the evaporation of brine, the fuel cost the mills nothing and the cost of salt-production was confined to pumping the brine and handling the salt, which found a ready market at high prices.

2 Salt-manufacturing establishments increased in number, and brine and rock-salt beds were discovered in many states; the market became glutted and prices fell; and cooperage became so scarce that the barrel cost more than the salt it contained. Manufacturers who were not entirely driven out of the market were compelled to adopt more efficient and cheaper methods of producing salt.

3 Finding themselves in this position about six years ago, my company, after investigation, installed mechanical rakers and conveying apparatus, a class of machinery then new to the market. Our apparatus is essentially that described by Mr. Willcox. After correcting the defects incident to a new departure, we found ourselves in possession of excellent salt-handling machinery.

4 The old-style method had required 10 men to lift by hand and store the product of 8 grainers; by the present mechanical methods, 4 men could handle 10 grainers. In our experience a mechanically

lifted grainer makes no more salt per day than a hand lift, the raker advantage being altogether one of labor-saving.

5 One of the chief requirements in the design of a salt raker is that all parts of the machine that come in contact with the hot brine be entirely submerged in it, as the combined action of brine and air will rapidly destroy the apparatus.

6 The pioneer work of Mr. Willcox in exploiting the reënforced concrete grainer, marks an epoch in the engineering department of

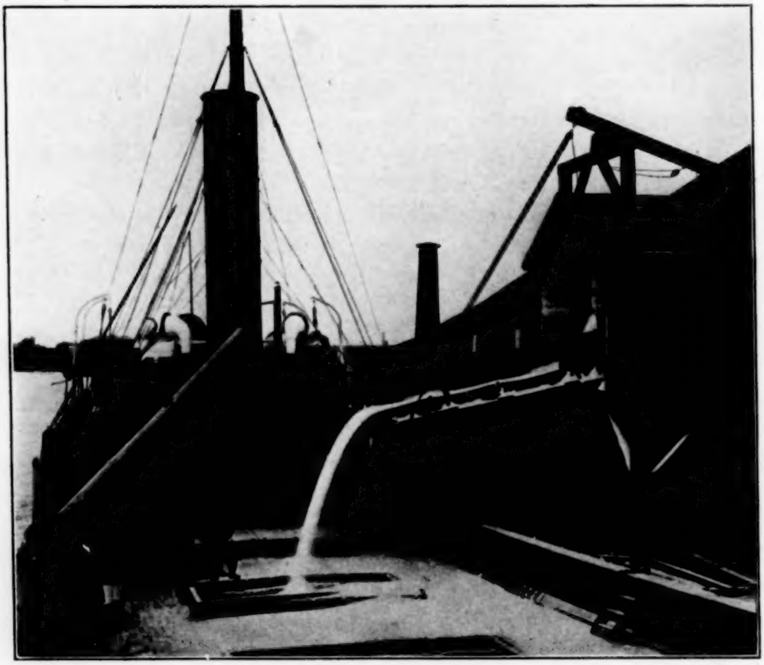


Fig. 1 Loading a Boat with Salt at the North American Chemical Co., Bay City, Mich.

the salt industry. During the construction of these grainers at Saginaw the salt fraternity looked on with distrust, thinking that the variation in brine temperature would soon destroy the work, but the sand joint between grainer and foundation apparently takes care of this point.

7 The scarcity and cost of the high-quality lumber necessary for grainer construction makes the advent of the concrete grainer very welcome. Wooden grainers are hard to maintain and susceptible

to many deteriorating influences. For instance, a wooden grainer can be kept water-tight only by continuous use. It will leak if turned out two or three days for repairs or cleaning; this means the loss of brine and salt for perhaps a week until the leaks are taken up by a fresh deposit of salt.

8 The writer repaired a number of old wooden grainers a few years ago by lining them with 8-oz. duck, saturated with a high-temperature asphaltum compound. The melting-point of this compound is 260 deg., and a grainer seldom shows over 185 deg. The duck, which is $\frac{1}{16}$ in. thick when saturated, was laid with 6-in. laps and all joints covered with the compound and reënforced with strips of saturated duck. After the grainer was well lined another reënforcement of compound was spread over the bottom of the grainer on top of the saturated ducking, and a lining of inch-boards was nailed in the bottom and sides of grainer. This repair job has been running four years and the grainers are still tight, although the original planking is nothing but a bunch of loose fibers which can be pulled to pieces with the bare fingers.

9 As for handling, the belt method is doubtless undesirable with a small but continuous quantity of salt, owing to the liability of the belt-mechanism to collect dirt and spoil the salt. On the other hand, this method is ideal for handling quickly large quantities of salt, a condition exemplified in our salt works at Bay City, where we make salt by vacuum pan as well as by grainer. This salt is carried by belt from the drainage bins which first receive the salt from the vacuum pans to the warehouse bins, a distance of 350 ft., at the rate of 15 tons per hour. The belt is 14-in. operating at a speed of 250 ft. per minute. Five men do the work of handling this 100 tons or 700 bbl. of salt per day; without the belt we employed 9 men.

10 We also have a belt built in a trough across our wareroom floor, with another belt running up an incline at the end to overhang the Saginaw River; under this projecting end a lake-boat of any size can be moored; 30 or 40 men can wheel salt from the warehouse floor in wheelbarrows and dump it into this floor trough, and the salt is forthwith carried by the belt and landed in the hold of the boat. This loading belt is 24 in. wide, runs 300 ft. per minute, and will load at the rate of 150 tons per hour, the rate of loading depending altogether on the number of men at the wheelbarrows. Without this belt we could not load boats with bulk at all, because our warehouse floor is lower than the deck of a vessel.

11 The net result of our installation of salt handling machinery is as follows:

On grainer raking outfit, saved 6 men.

On vacuum pan outfit, saved 5 men.

A total of 11 men saved on handling about 1000 bbl. of salt per day, a cash saving of nearly two cents per bbl. of salt.

The interest charge on these improvements and the repair account come to a very small fraction of a cent per barrel.

The Author. As showing the relative advancement in the development of special salt works machinery in America and abroad, it is of interest to note that the German Government recently sent a commission of salt experts to this country, and as a result that Government has adopted this system and has authorized the building of a large salt works at Schoningen, which is in its essential features of design and equipment practically a duplicate of the plant described in this paper.

METAL CUTTING TOOLS WITHOUT CLEARANCE

By James Hartness, Published in The Journal for December

ABSTRACT OF PAPER

This paper sets forth a turning tool that is intended to cut without clearance.

It consists of a cutter and a holder so constructed as to allow the cutter a slight oscillatory freedom in the holder. The center line on which the cutter oscillates is substantially coincident with the cutting edge. The oscillation of the cutter about the center line does not affect the position of the edge, but it does allow the face of the cutter to swing around to conform to the face of the metal from which the chip is being severed.

The objects of this construction are to make possible the use of more acute cutting edges in order to reduce the cutting stresses; to equalize wholly or partly the unbalanced side pressure on the cutting edge; and to obtain a rubbing contact to prevent lateral quivering.

DISCUSSION

Mr. H. H. Suplee. This paper is entitled *Metal* Cutting Tools Without Clearance, and so my remarks may be a little off the exact subject. A number of years ago I used *wood* cutting tools without clearance in planing machines, for much the same purpose as Mr. Hartness has used his. In the wood planing machine, in order to avoid chattering and get smooth work on high grade hard wood flooring, and work of that kind, the knives were set as truly as possible, and then the cutter head revolved, and a revolving emery wheel was carried back and forth, grinding off the back edge of the knife, to maintain the true cylindrical surface of the revolving cutter head. So a flat edge, or rather a cylindrical edge, was obtained for the back part of the cutting knife. The cutter produced remarkably smooth work. All the knives were doing an equal share of the work, and the chattering and quivering common in many planing machines was almost entirely removed. The real difficulty was that the edges of the tools got so hot after a certain period, that their temper was drawn.

The Author. Mr. Suplee's remarks regarding the use of no-clearance blades in wood-planing machines are full of significance. The question of heat generated by the friction of the riding-contact is one that will require more time to settle definitely. The results may not be the same for different materials. We would expect an important difference between steel and castiron. The extra heat generated in wood-planing may have been due to some or all of the cutters having acquired a negative clearance. The springing of the material or the head would cause substantially a negative clearance. Excellent results are now being obtained by this scheme by the planers made by the S. A. Woods Company of Boston.

2 The whole subject is so full of unsettled points that the author hopes others will be induced to contribute papers, especially along the lines of acute cutting-angles, clearance, and cooling or lubricating solutions.

LIQUID TACHOMETERS

By Amasa Trowbridge, Published in The Journal for Mid-October

ABSTRACT OF PAPER

The principle on which the Veeder liquid tachometer acts is that the pressure developed by the centrifugal force of the liquid when the instrument is running at a certain speed is a definite quantity. This pressure forces liquid up the indicating tube and is balanced by the pressure due to the height of the column of liquid in the tube. The indications of the instrument depend entirely on the velocity, and do not change if wear is eliminated. It makes little difference whether the liquid employed be something heavy like mercury, or light, as alcohol. The indications are essentially the same for both liquids.

DISCUSSION

Mr. H. H. Waitt. I think the makers of this instrument are to be congratulated on perfecting a device which is very beautiful from a scientific standpoint and which will probably find a large field of application. It offers remarkable possibilities in the way of accurate speed regulation when combined with some sort of electrical or mechanical relay device. With an electrical device one might expect to control the speed within one-tenth of one per cent.

2 The simplest form for such a device would be contacts acting directly on the mercury column. Judging from the experience of past years with arc-lamp circuit regulation, considerable attention would be required to maintain this form of contact in proper condition. Some improvement would probably be made by having a layer of oil on top of the mercury; at any rate it would seem that such a device could be used when accurate regulation was necessary.

3 The difference in head of the indicating column does not provide enough force to operate much regulating mechanism directly. For example, with a 30-in. mercury column, 1 per cent rise in speed would correspond to about 0.6 in. increase in head, which would mean about 0.3 lb. per square inch so that a considerable area in the

shape of a piston, float or diaphragm would have to be used to move anything but a relay mechanism. It is interesting to note that one of the Rateau Turbine patents shows a small centrifugal pump for governing the speed of the turbine through the intermediary of a diaphragm.

4 This instrument could be used as a very simple, accurate and reliable speed-limiting device, by having the mercury or other conducting column on the suction side of the pump; in this could be immersed a contact which maintained a circuit holding a safety-throttle open (in the case of an engine), or in the case of an automobile some part of the ignition circuit could pass through the contact. If the speed rose above a certain amount, the mercury would be drawn away from the contact and the safety device would operate, which would also be the case if the contact were defective or if anything else happened to the circuit. It is conceivable that such a device would not be welcomed with enthusiasm by drivers of fast automobiles.

5 Mr. Trowbridge speaks of the effect of viscosity. I would like to inquire if this effect has been accounted for. At first glance I thought it might be due to the velocity-effect of the impulses due to the individual paddles. These impulses would probably be more marked with the liquids which flow more readily. There would, therefore, be a component of the total pressure which would be proportional to the square root of the mean square of the velocity impulses. The trouble with this hypothesis is that it is not in accord with the facts, and we must look for some other explanation.

6 Possibly it may be on account of the more viscous fluids flowing less readily through the leakage paths so that the static head is not as much reduced by the internal drop due to the resistance.

7 In using the tachometer as an instrument of precision, several points should be borne in mind. In the equation for the indication of the instruments, the force of gravity enters into one member and not into the other. That is, the centrifugal force is proportional to the mass, while the height of the column is dependent on gravity. However, as the maximum range of the force of gravity under ordinary circumstances is only about one-third of one per cent, this would not require a correction in the ordinary uses of the instrument.

8 It is possible that considerable error might come into the observations of pulsating speeds on account of the throttling of the fluctuations at the constricted portion of the column. Unless the approach to the constricted portion and the discharge from this

place be shaped so as to avoid it, we see that we get the dynamic head of the impulse in velocity caused by the pulsations in speed. This then may give us an indication proportional to the squares of the velocity instead of to the first power of the velocity. This exaggeration of the average velocity may not amount to anything in practice, but it might be worth remembering, in case of testing such machines as explosion engines without much fly-wheel effect. The exaggeration of the mean velocity, in a rough way, would be proportional to the square of the variation of the ratio of the minimum to the maximum velocity.

9 It would be interesting to know how much power is absorbed by the tachometers and whether there is much heating of the fluid, and also if the instrument is arranged to compensate for this.

Mr. H. G. Reist. This paper is of special interest to me because I have watched the development of the instrument; and now for a number of years the company with which I am connected have used a large number in testing machinery to determine instantaneous readings of speed, as well as in the laboratory for calibration purposes. Owing to its stability, the liquid tachometer is particularly valuable for the latter purpose. A long tube may be applied to the instrument, enabling the observer to read with extreme accuracy.

2 This form of tachometer is very sensitive; yet it can be read accurately, because the column of liquid is stationary and not subject to the vibration peculiar to the needle of mechanical tachometers. There are no errors due to friction or backlash, as is the case with some mechanical instruments. These errors are frequently not serious, even on mechanical tachometers, but sometimes it is desirable to determine speed accurately and it is very difficult, with mechanical instruments, to make such a measurement closer than about 1½ per cent.

3 The only possible error in using the Veeder tachometer is in estimating the position of the column of liquid in the tube. This, however, can be done with considerable precision. We find in practice that the instruments are fairly permanent, the only variation being a slight change of zero due to evaporation of the alcohol used in the instrument. A variation of $\frac{1}{32}$ in. on either side of the zero-point does not, however, affect the calibration appreciably. It is probable that the substitution of oil for alcohol would reduce even the slight care now required. On account of this evaporation, the Veeder instruments need a little more attention than others, but the precise results obtained more than make up for the extra care.

Prof. W. F. Durand. In connection with the statement of the theory of this instrument, attention may be called to the formula for centrifugal force. The use of the formula seems to imply that the pressure head developed by centrifugal pump is $v^2 \div gr$, or in other words that the pressure developed at the periphery of the rotating water is measured by the centrifugal force due to a mass W, rotating with velocity v at radius r, or by $Wv^2 \div gr$, the common formula for centrifugal force. It is well known, however, that the pressure-head developed by a rotating cylindrical body of water is due to an integration of elementary centrifugal force effects distributed throughout the entire body of water, and that the result of this summation at the periphery is to give a pressure head measured by $f\ (v^2 \div 2g)$, where f is a factor drawn from experience. This difference has no practical influence on the relation of the indication of the instrument to the speeds of rotation, since in any given instrument the radius is constant, and the pressure-heads will vary as the square roots of the rotative speeds.

2 The main point indicated is that the pressure-head depends primarily on the linear peripheral velocity, and except for secondary influences is independent of the radius except in so far as the latter is a factor of the linear velocity.

3 Some twenty years ago, I began introductory experiments on certain forms of fluid tachometers, which were interrupted by a change of location and have not been taken up again. One indication of these experiments was the dependence of the readings of such instruments on the physical characteristics of the fluid, especially as regards viscosity, a point which has been examined in the Veeder instrument, with promise of further study.

4 One point comes out very clearly in the study of such instruments, that in order to be reasonably independent of fluctuating conditions the indication of the instrument should be made by the same liquid as that employed for producing the primary pressure-head. In this manner, as pointed out by the author, the density-factor may be eliminated and the indication made practically independent of this factor. There seems to be, however, one possibility of slight error due to the imperfect elimination of this factor. After the instrument has been at work for some time, the liquid within the case will have become somewhat elevated in temperature, with corresponding decrease of density. There is, I believe, no replacement of the liquid in the column, hence there may arise in this manner a permanent difference of temperature between the liquid within

the pump and that in the column. Unless this difference remains sensibly constant, and unless the calibration of the instrument includes the influence of such difference, such temperature effect might introduce slight error. It would be interesting to learn if indications of any such error have been observed.

The Author. In regard to the use of a tachometer for regulating the speed of a machine, I agree with Mr. Waitt that the tachometer should be used only as an automatic switch or controller. The use of a film of oil on the mercury column was tried in the testing-machine described but it was discarded in favor of a condenser. When the condenser is used, very little trouble is encountered from the spark caused by the breaking of the circuit; when a film of oil is used on top of the mercury column, this leaves a thin skin of oil over the point which makes contact with the mercury, and the current must break through this insulation before making contact.

2 The reason for the more viscous liquids giving a higher column than the less viscous is probably found in the smaller flow through the leakage-paths. This is one of the points that we have still to investigate.

3 As pointed out by Mr. Waitt, a slight error in the indications of a liquid tachometer is possibly due to irregularities in the speed. The instrument may indicate slightly high because the mean sum of the squares of the velocities of the paddle is greater than the square of the mean velocity. These errors are fortunately very small, and it is seldom required to indicate the speed of an irregularly driven shaft very closely.

4 The power required to drive one of these tachometers is extremely slight, so small, in fact, that we have not yet measured it. Some notion of the ease with which they are driven can be obtained from the fact that one of the automobile-type instruments was driven for more than 46,000 miles on the Boston Elevated Railroad by a wire-wound flexible shaft of $\frac{1}{8}$ in. diameter. This shaft had to make some turns and hence would have given out long before completing this run had much work been required of it. It has not yet broken.

5 The loss of liquid from evaporation, noted by Mr. Reist, amounts to almost nothing, and the variation in the amount of liquid which is permitted in the instrument by the use of the displacement plunger is sufficient so that no liquid needs to be added for a long while. The substitution of oil for alcohol makes the action of the instrument

slightly slower, so that the alcohol has been considered the most satisfactory liquid. Kerosene oil, in which is used in the automobile instrument, adheres somewhat to the glass and makes the indications slightly less clear than when alcohol is used. It is also objectionable on account of its penetrating quality.

6 Referring to Prof. Durand's remarks on the pressure-head developed by centrifugal pump; while there is a factor introduced by the integrating of the elementary centrifugal forces, this, as he points out, has no practical effect in these instruments. In an instrument where the paddle has short blades in proportion to the radius of the hub, more care has been found necessary to keep the instruments accurate. A fair length of blade must be used or the inaccuracies of the instrument have some effect on the indications.

7 The point raised by Prof. Durand in regard to the change in the density of the liquid has not so far proved to be very important. The liquid does heat very slightly in the paddle-case, but it can be thoroughly mixed with the cooler liquid in the column by stopping the instrument for a few seconds, allowing the column of liquid to drop. In certain experiments to determine the effect of the rise of temperature it was found that by considerable heating of the pump without heating the liquid column some change could be made in the indications of the instrument. This change was not serious unless the range of temperature was very much greater than has ever been encountered by the heating of the instrument from its own work. The instrument heats very little when alcohol is used; and even with a heavier liquid, like mercury, no serious difficulty has been encountered from this cause. Rise in temperature due to using it in a warmer room than that in which it was calibrated, has no sensible effect on the indications, as the density of the liquid is practically uniform throughout the instrument in this case.

8 In setting up any of these instruments, they should be so arranged that they can be stopped occasionally, allowing the liquid to come to rest. It can then be readily observed whether the setting to the zero mark is correct, and the liquid from the column can become mixed with that in the instrument, thus obviating any trouble from change in density.

INDUSTRIAL PHOTOGRAPHY

By S. Ashton Hand, Published in The Journal for November

ABSTRACT OF PAPER

Industrial photography calls for results of the highest order.

The lens used should be of long focus; never shorter than the diagonal of the sensitive plate.

Machinery to be photographed should be painted a "flat" drab color. Parts in shadow should be painted a lighter shade than more prominent parts.

Light should come from one direction only, and at a downward angle of about 20 deg. from the horizontal.

In focusing, the points of sharpest focus should be midway between the center and the edges of the ground glass.

No matter how much the camera is pointed up or down, the ground glass should always be vertical.

Exposure should be ample. An under-exposed plate can never show what the light has never recorded upon it.

DISCUSSION

Dr. C. J. H. Woodbury. I am a photographer by proxy only, but will give my experiences with bright metal work, in having photographs made of machinery.

2 Many years ago, the late John C. Hoadley, a member of this Society, extensively advertised the portable steam engines made by him at Lawrence, Mass., by means of photographs for which the finished metal work was whitewashed, giving excellent results.

3 In photographing silver, shaking a bag of cheese cloth containing face powder near the articles, until the white dust has settled upon the object and cut off the glare, has given more satisfactory detail than the putty process, as putty contains yellow tints not visible to eye, but giving in a photograph noticeably darker effects than those of silver. Where silverware is extensively photographed, the articles are arranged, and the camera focused; then they are removed to a refrigerator, and chilled; on being replaced in position the moisture

from the air condenses upon the cold surfaces, and the photograph is taken before it accumulates into drops.

4 Photographers claim to obtain the best results with small diaphragms, moderate lights and long exposures. Interiors containing much detail can be photographed by darkening the room, drawing the curtains, closing the blinds, and giving an exposure of from one to two hours, with a small diaphragm, after which the camera is closed, and the blinds are opened and the curtains raised to admit the light, and a snapshot is taken, using a larger diaphragm; the result will be a photograph revealing detail to an extent not possible with single exposure.

5 A public building decorated for a civic celebration with flags, bunting and many incandescent lights was successfully photographed by first taking an insufficient exposure during the day, and then without moving the camera taking another exposure during the electric illumination in the evening, with the result that the building was faintly shown in the photograph much as it appeared to the observer in the evening.

Mr. Henry B. Binsse. I would like to take issue with the author about one point—he says that the machinery should be painted a drab color. Of course I have not had as much experience as he, but the best results which I have ever obtained have been from painting the machines a salmon pink. The effect of drab color was to make a machine look flat, while if painted a warm color the parts stand out more; you get more of a picture.

Mr. Charles W. Hunt. In connection with Mr. Hand's paper it may be of interest to submit a record of some experiments made by the writer for the purpose of estimating the proper time of exposure for dry plate photography. The following tables are based on this series of experiments. As a preliminary step the altitude of the sun was calculated, for each hour of the day from sunrise to sunset, on the first day of each month in the year, and the results plotted.

2 In June 1905 a series of exposures was made with a Watkins exposure meter at each hour of the day, and a tentative table of the relative exposure time for each hour from sunrise to sunset was made. Using this tentative table a series of similar exposures was made with dry plates. The plates were each developed the same length of time and in the same strength of developer. From these tests the tabular time was corrected.

3 A table was then made giving the estimated time of exposure for each hour of the first day in each month in the year, basing the time of exposure largely upon the tests and the altitude of the sun in the different months. During the ensuing year this table was tested from month to month, and revised as experience indicated, in order to get the best attainable negative at any hour of the day in any month of the year. The present table is derived from the results of the above tests, with the formulae corrected to correspond with exposures made on Eastman films of the current year (1908).

4 The time for a theoretically perfect exposure, that will result in the best printing negative that the subject will give, cannot be expected from any formula that takes into consideration only the most prominent factors affecting the problem. These rules may, however, be expected to give a reasonably close approximation to a perfect exposure.

5 In making exposures, where it is unusually important to secure a good negative, and the exposure cannot be repeated, proceed as follows: Compute the proper exposure by the formula. Then give three exposures: (1) The first exposure with time as computed; (2) The second with one-half the computed time; (3) The third with double the computed time.

6 For less important cases, but where great uncertainty exists as to the proper time of exposure, proceed as above, but make only two exposures, the slowest and the fastest, omitting the computed time exposure. The latitude of the plate will give a satisfactory negative if the theoretically perfect time of exposure lies within very wide limits.

7 An exposure should not be made in a fog, and in hazy weather only of nearby subjects. Good negatives may be made during a shower if the weather is otherwise clear. Generally, if contrast in the negative is desired, "underexpose;" if definition in the shadows is wanted, "overexpose." When in doubt, it is safer to "overexpose." Stops number 64 or 128 are excellent for general outdoor exposure; number 32 or 16 for indoor work. If it is desirable to emphasize a specific part of a machine focus carefully with a large stop and shorten the exposure to correspond with the stop.

8 The following formulae and tables are based on normal light conditions and ordinary subjects. If either or both are abnormal, the operator must make an allowance in the duration of exposure as computed by cœfficients from Tables 1, 2 and 3.

TABLE 1 COEFFICIENTS (A) FOR PHOTOGRAPHIC EXPOSURES IN THE LATITUDE OF NEW YORK

Month	Hour of the Day				
	7 to 8 or 5 to 6	8 to 9 or 4 to 5	9 to 10 or 3 to 4	10 to 11 or 2 to 3	11 a.m. 12 m. 1 p.m.
January—December	..	2	4	6	7
February—November	..	4	5	7	8
March—October	2	5	6	8	12
April—September	4	6	9	12	16
May—August	5	8	12	18	28
June—July	7	10	16	24	32

TABLE 2 WEATHER COEFFICIENTS (B)

Clear, sunshiny weather	1.
Floating, white fleecy clouds	1.
Overcast, but a light day	1.5
Cloudy, dull day	2.
Lowery, heavy clouds	4.

TABLE 3 SUBJECTS: COEFFICIENT (T)

Shop interior, dark and poorly lighted	1000.
" machinery fairly well lighted	400.
" machinery placed near a good window light	150.
Machinery under sheds with one side open, or covered areas	15.
" outdoors to give details in the shadows	2.
" " general views	1.5
Groups, or portraits outdoors	1.
Buildings and nearby landscape	1.
Distant structures or landscape views	0.5

TIME EXPOSURE

9 Assume a stop suitable for the subject and call it H; then the seconds to expose will be:

$$\frac{H \times B \times T}{32 \times A} = \text{seconds exposure for an } H \text{ stop} \qquad (1)$$

BULB EXPOSURE

10 A "quick" bulb exposure is a time exposure of about $\frac{1}{5}$ to $\frac{1}{4}$ sec. To compute the number of the lens stop use the formula:

$$\frac{8 \times A}{B \times T} = \text{stop for a "quick" bulb exposure} \qquad (2)$$

Mr. H. H. Suplee. I have used with a good deal of success flashlight powder in connection with daylight, to emphasize the dark part of a machine, where it is impossible to get proper light on small

parts; a small flashlight discharged out of the field so as to reflect the light on the machine will supplement time-exposure and bring out the parts which would not otherwise be visible.

2 My own experience is that it is almost impossible to compute the time of exposure with any accuracy when there are so many variables. The amount of light has to be estimated always, and in my opinion the best plan is to use a moderately slow plate and give a long exposure. If the plate is developed with a long development, you will get detail which you could hardly calculate or compute by attempting to work at any definite time of exposure.

3 Photography has been put to very important uses in engineering work lately, in connection with moving picture machines. We saw last night moving pictures showing the operation of a modern flying machine. They have also been used to show the operations in the workshop, by mounting the machine on a car, and running the car at moderate speed, and the picture machine at full speed, through the shop, the result being a lesson on shop practice, which can be used in a course of lectures more effectively than any other possible means.

4 Several years ago I suggested the possibilities of the moving-picture machine as a means of engineering investigation. It is possible to run the machine very rapidly and get a succession of pictures that represent a very short duration in making the strip. This film can then be run through the picture machine at a speed much slower, with the result that an operation so quick as to be imperceptible to the eye is slowed down so as to be readily examined. I have made no experiments, but I think it is possible, for instance, to photograph a fracture of a piece in the testing machine, occupying only a few seconds at the critical moment, so rapidly in an artificial light on a continuous film, that it could later on be thrown on the screen and slowed down so that the whole sequence of rupture would be plainly visible. Many other applications will suggest themselves for studying phenomena altogether too rapid to be examined by the eye. I believe these matters are likely to be applied to the laboratory, and possibly to workshop operation.

5 Another use is that of keeping a record of the progress of work, a record which cannot be disputed; this is often applied by contractors and builders day by day, to keep a record of the operation of buildings, etc.

6 Another use of photography, which I employed a number of years ago with a good deal of success, is for preserving a record of valuable drawings in a limited space. A full set of complete double

elephant drawings of a whole line of machines were photographed down to $6\frac{1}{2}$ by $8\frac{1}{2}$ in., and a set of prints were made, which formed a parcel that could readily be put away in a safe deposit drawer. In the case of the destruction of the originals by fire, these could be enlarged back to the original dimensions, and thus a perfect fire insurance is obtained for a valuable set of drawings.

Dr. Woodbury. As an example of the use of photography to represent the operation of machinery, the makers of a magazine loom which contained many new and important features had a series of moving pictures taken showing one of these looms in operation, and half-tone plates were engraved from these continuous negatives.

2 These half-tones were printed on a series of small cards hinged together and placed in a small portable device resembling a stereoscope so that by turning a crank the pictures were revealed in rapid sequence, and one could see the loom operating at normal speed, or by turning the crank very slowly, the various operations of the mechanism could be noted in a way not possible of observation when the machine was in its normally rapid operation. Many of these sets of illustrating devices, which were merely those used for a long time in moving-picture shows, were placed in the hands of salesmen and were doubtless the cause of the rapid introduction of this loom.

3 At the other extreme, photography has been used to analyze very slow motions, as for example, a growing plant, which was photographed day after day, the results when put in a moving-picture machine giving the method of extension of the plant. About twenty years ago, the late Col. W. E. Barrows, a member of this Society, built a mill at Willimantic which contained a great many novel and original features, and weekly photographs were taken and sent to members of the board of directors to illustrate the progress of the work.

4 When one of the recent half-encyclopedias and half-dictionaries was in process of preparation, a great many of the more valuable manuscripts were photographed down to about the size of a postage stamp for a page, and the negatives of the photographs were kept in a safe-deposit vault. These instances merely indicate that the dry-plate method of photography has placed in the hands of laymen without the expensive plant and skill of the old-time photographer, the ability to show things, as the author stated, as "they are" and

to give a true record of the progress of work or the existence of any forms of machines or other objects.

Mr. Ambrose Swasey complimented Mr. Hand on his photographs of machinery and interiors and pronounced the paper of very great value to the Society.

The Author. The use of face powder as well as of refrigeration, in photographing articles of silver, are both new to me, but I think either would be excellent for the purpose.

2 Whether small diaphragms, moderate lights and long exposures produce the best results, will depend on the amount of contrast in the subject to be photographed. Size of diaphragm and length of exposure must be governed entirely by conditions and no set rule can be given governing their relation to each other.

3 The methods of making photographs of illuminated buildings and of interiors, have been successfully practiced for years.

4 Painting machinery a salmon pink would be all right for photographing in a very subdued light. I have never-had any trouble in getting plenty of contrast with machinery painted a drab color. In fact, I have to give long exposures to avoid too much contrast.

5 Mr. Hunt has gone into the matter of exposures very thoroughly, but he has left out of his table of weather-coefficients one very important condition of light, and that is its color. A day may be clear and sunshiny, but if the light is yellow an exposure of four to ten times that deduced from his formula may be necessary. Exposure-tables are all right for the first guess, but any one of the many factors to be taken into consideration may so upset all other calculations that experienced photographers have never used them to any extent.

6 The use of flash-powder in connection with daylight exposures to emphasize dark parts of machines, is all right, providing there are no projecting parts to cast shadows. Shadows cast by flash-light are very intense.

7 For the successful use of a moving-picture machine in the shop, the shop must be a very well-lighted one, or a lens with an enormously large opening in proportion to its focal length must be used.

8 I have been asked if I have had any experience in showing by photography whether buildings were plumb or not. An interesting experience that I had several years ago will serve as an answer.

9 While the Bourse Building in Philadelphia was in course of construction, a man asked me to photograph the steel structure which had just been erected. I found he was one of the kind of men who write for the daily press on all sorts of civic question, signing himself "Pro Bono Publico," or something of like nature. By great difficulty we obtained a position on top of a building where we could get a good view of the steel work. After getting my position and focusing the camera, I asked what he wanted the photograph to show. He said the steel work was out of plumb and he wanted the photograph to show it. I turned to him and said, "Which way do you want it out of plumb? I will make it either way you want it."

POSSIBILITIES OF THE GASOLENE TURBINE

By Prof. Frank C. Wagner, Published in The Journal for Mid-October

ABSTRACT OF PAPER

In order to reduce the temperature of the gases at the turbine wheel, either an excess of air or water injection may be used. The relative efficiencies of the two methods is shown to depend upon the amount of compression used, and the efficiencies of the turbine and the air compressor. The work required to compress the air may be materially reduced by using two-stage or three-stage compressors with intercoolers. It appears that with high compression the gasolene turbine may be expected to give efficiencies comparing favorably with the reciprocating gasolene engine.

DISCUSSION

Dr. Sanford A. Moss. Theoretical computations such as those of the present paper are very fascinating and the ground has been threshed over many times. The case which Professor Wagner considers is customarily called the "Brayton" cycle.

2 The cycle originally proposed by Diesel had addition of heat at constant temperature, not at constant pressure. The Brayton cycle, with cooling by excess of air or by water injection, is considered in most of the references given below. Theoretically, this cycle is not the most perfect one and much better results are computed by some of the authors with addition of a regenerator.

3 Complete computations of the whole subject, with cooling by air excess only, and without allowing for losses of any kind, were given by Dr. Harvey N. Davis, in an article entitled P Q Plane for Thermodynamic Cyclic Analysis, Proceedings of The American Academy of Arts and Sciences, vol. 40, no. 19, April 1905. Other computations similar to those made by Professor Wagner have been given in many papers of which the following may be mentioned: Scientific Investigation into the Possibilities of the Gas Turbine, by R. N. Nielson, Proceedings of the Institute of Mechanical Engineers (London), October 1904, p. 1061; The Gas Turbine, by Alfred Barbezat, Schweizerische Bauzeitung, about 1904, copied extensively

in the Continental press; The Gas Turbine, by M. L. Sekutoweitz, Memoirs of the Society of Civil Engineers of France, February 1906.

4 An exact analysis of the subject is manifestly impossible, owing to our absolute ignorance of the specific heats, densities, amounts of dissociation, nature of expansion, corrections to perfect gas laws, etc. Therefore, various assumptions have to be made, and the validity of the efficiencies and temperatures computed is very uncertain; and no conclusions should be drawn regarding the differences with one set of initial conditions and another. The temperatures in particular are open to serious question and it is highly doubtful if the computed weights of water, air, fuel, etc., would give the computed temperatures of the paper.

5 As I understand the computations, Professor Wagner handles the expansion of a mixture of superheated steam and products of combustion in the following way:

6 He assumes a final temperature of 1200 deg. and computes a maximum temperature of 2225 deg. for expansion between the given pressures, under the assumption that the mixture of steam and products of combustion acts as a perfect gas with a specific heat ratio of 1.39.

7 He then computes the respective quantities of products of combustion and steam by assuming 0.25 as specific heat of the products of combustion and 0.47 as the mean specific heat of the superheated steam, the latter figure being on the basis of Professor Thomas's experiments. The temperature of the steam at the end of the expansion is then computed by assuming it to be a perfect gas with a ratio of specific heats of 1.31. This comes out 1340 deg., a number different from the originally assumed figure of 1200 deg.

8 The work done by the products of combustion is found from the initial and final temperatures of 2225 and 1200 by assuming a perfect gas with specific heat of 0.25. The work done by the superheated steam is found from the initial and final temperatures of 2225 and 1340 by assuming a perfect gas with specific heat of 0.47.

9 Probably the assumptions made are as good as any other set of assumptions in the light of present knowledge. It is also possible to handle the computations in other quite different ways. The adiabatic expansion of a mixture of two perfect gases is the same as the expansion of a perfect gas having specific heats which are between the actual specific heats in ratio of the respective proportions. In view of the assumptions, it does not seem proper to draw any definite conclusions from the numerical results given in the paper.

PROF. CHARLES E. LUCKE. One of the primary elements in any gas turbine is the nozzle which transforms the heat into work. Understanding this, I made an investigation of some nozzles some time ago and ever since have been investigating nozzles more or less diligently.

2 The value of a nozzle as a heat transformer, when handling a perfect gas, is directly measured by the extent to which it can cool the gas in giving it velocity. So there is no better measure and none more easily applied, than the difference in temperature between the gases entering the nozzle and the gases at the point of maximum velocity. Unless nozzles can be shown to have the power of very materially cooling gases of a given pressure drop by velocity imparted, a successful turbine can never be built. I have never succeeded in making the temperature drop exceed 18 per cent, judging by the methods of determination available, of what would have been accomplished by using a cylinder. In view of that fact it seemed to me not worth while to spend time clothing the rest of the mechanism in metal.

3 I urge those interested in this problem to consider it from the viewpoint of simple experiment. Do not lay out a lot of compressors and turbine wheels and design the details of the bearings; do not make a lot of patent drawings, but go into a laboratory, which may be only your cellar, and make actual compressed air experiments: anybody can produce enough with the water pressure in the house; and when a nozzle has been formed that, working with a moderate pressure, say 30 lb., will freeze water on itself, then it will be time to talk about gas turbines as engineering possibilities removed from speculation.

4 It is quite likely that with a mixed gas turbine, involving the addition to the fixed gases of some steam formed by the internal combustion, different conclusions will be in order. Concerning this sort of possibility there are no data on record beyond some figures for complete machines, which it is impossible to analyze. The laws of expansion for mixed steam and gases can be derived for various ratios of weights and measures found for the effectiveness of the heat transformation into work. The perfection of any nozzle will be determinable by comparing the measurements of effects made by means of it with those that might be computed by the thermodynamic laws. Whenever this comparison shows the nozzle to be as good or nearly as good a transformer as a cylinder, then it will be time to consider a form of mechanism to carry out the turbine

action completely, that is to say, design the turbine; but as I see the question this problem should not be approached before data of the above sort are available.

PROF. WM. KENT. I have given a great deal of study to this subject and, in my book on Steam Boiler Economy, in the chapter on Combustion, I have given an argument in favor of the higher heating value of 62,000 B.t.u. for hydrogen in all calculations connected with fuel. The argument from the lower heating-value is that we cannot utilize all of the heating-value in any process in which the gases escape into the atmosphere at a point higher than 212 deg., as in most producers and gas-generators. But if we are going to subtract from the higher heating-value, or 62,000, a certain number of heat-units, and say the heating-value is 57,000 because we cannot recover the latent heat of the water-vapor, why not go further and say we cannot recover all the heat of nitrogen of the air used in burning hydrogen and the heat in the excess air, due to its escape at a high temperature?

2 I have shown in that chapter several different lower heating values of hydrogen, depending on the composition of the gases of combustion and on the temperature at which the gases are supposed to escape. If we start with hydrogen at 62,000 B.t.u., and call that 100 per cent, then in making a heat balance a certain percentage is transformed into work, say 20 per cent, a certain percentage is lost in the sensible heat of the gases escaping in the chimney, say 30 per cent, perhaps 10 per cent is lost in latent heat of the water-vapor escaping, and the balance is lost in the cooling-water and in radiation.

3 Why should we not charge all these losses against the 62,000 making the efficiency 20 per cent, instead of first deducting the assumed heat lost in the latent heat of the water-vapor, 10 per cent, and reporting the efficiency as $20 \div (100-10) = 22.2$ per cent? As most of the existing literature on combustion deals with the higher heating-value, why should we bring confusion into the subject by using the lower value?

4 In regard to Professor Hollis' curve showing the relation between hydrogen and total volatile matter; in that same book of mine there is a curve showing the relation of the heating-value of the combustible portion of coal to the percentage of volatile matter in that combustible. In this curve it will be seen that the heating value of coal is the highest (15,750 B.t.u. per pound) when the volatile matter is 20 per

cent of the combustible corresponding to Pocahontas or other semi-bituminous coals. In anthracite the combustible part has a heating-value a little higher than that of carbon, say 14,900.

5 Now, studying the results of Mahler's work in France, I plotted his figures around this curve, and found the whole field of his work covered a very narrow field, in the coals having less than 36 per cent of volatile matter in the combustible, all the observations plotting near to the curve, but beyond 36 per cent the field broadened so that some coals high in volatile matter might have a heating-value considerably higher or lower than that indicated by the curve. In other words, the relation between the heating value of the coal and the volatile matter up to nearly 40 per cent may be expressed by a curve, with an error of not over 1½ per cent as a maximum; beyond 40 per cent, however, coals of different districts may have a very wide difference in heating-value, although their percentage of volatile matter as shown in the analyses may be the same. The difference is due to the different percentages of oxygen in the volatile matter in these several coals, the higher the oxygen the lower being the heating value.

THE AUTHOR. Referring to the remarks of Dr. Moss, I compared the cycle of the turbine considered with the Diesel engine, not because Diesel was the first to use such a cycle, but because the Diesel engine is the best-known example at the present time.

2 It was not my purpose to present a complete discussion of the gas-turbine, but, as stated in the opening paragraphs, to show how the several methods of reducing the temperature of the gases were affected by variations in the efficiencies of the turbine and the compressor.

3 The statement given by Dr. Moss of my method of calculating the expansion of a mixture of superheated steam and products of combustion is substantially correct. The mixture would be at some temperature between 1200 and 1340 deg.

4 The change in the value of the efficiency produced by using a somewhat larger proportion of water, so as to bring the final temperature of the mixture to exactly 1200 deg., amounts to about one-half of one per cent. It hardly seems worth while to attempt such refinements in the calculation.

5 It may be true that more accurate determinations of the specific heats at high temperatures will change the particular values obtained for the several efficiencies. I hardly think, however, that the

relative efficiencies of the different methods of cooling will suffer material changes thereby.

6 The practical value of the calculations I have presented lies in showing what conditions of operation should be avoided. It appears that with low compression and inefficient turbine or compressor, a zero efficiency is to be expected. Some experimenters seem to have obtained this result.

7 Professor Lucke states that he has never succeeded in making a perfect gas cool itself materially in free expansion. I should be very much surprised if he had succeeded. So long as no energy is taken away from the gas, how can it be cooled? In free expansion of a perfect gas, the external work done upon the gas as it issues from the higher pressure is exactly equal to the external work which the gas must do in making room for itself at the lower pressure.

8 In the Engineering Magazine for August 1906, Professor Lucke describes the experiments which I suppose he has in mind when he speaks of 18 per cent as the maximum ratio of the temperature drop, as compared with expansion in a cylinder, that he was able to obtain. In these experiments the air was used to drive a Laval turbine-wheel, and its temperature was measured after it had substantially come to rest in the exhaust. The proper interpretation of the experiments is that an amount of energy corresponding to the observed drop in temperature was abstracted by the turbine-wheel. The tests show nothing at all regarding the temperature of the air as it left the nozzle or as it passed through the wheel.

9 The expansion of the gases in the nozzle of a turbine is not a case of free expansion. The gases attain a very high velocity at the expense of the heat-energy, and are correspondingly cooled. If the energy due to this velocity is delivered over to the turbine-wheel as mechanical work, then the gases will remain cool. Whatever energy is not converted into mechanical form in the wheel is transformed back into heat when the gases are brought to rest.

10 It is not to be expected that the full lowering of temperature corresponding to adiabatic expansion will be obtained practically in a nozzle. The friction work in the nozzle will be converted into heat, and tend to raise the temperature of the gases. So also will the friction work at the vanes. But these effects ought not to be greater than in the corresponding parts of the steam turbine.

A METHOD OF OBTAINING RATIOS OF SPECIFIC HEAT OF VAPORS

By A. R. Dodge, Published in The Journal for Mid-October

ABSTRACT OF PAPER

A method of obtaining ratios of specific heats is given which does not involve the use of available steam tables conceded to be too inaccurate for such investigations, nor a condition in which the steam is presumed to be without moisture or superheat.

This method is based upon the expansion of initially superheated fluid in a throttling calorimeter, and tables are included showing data for steam.

DISCUSSION

Dr. Harvey N. Davis, who had been particularly interested in Mr. Dodge's work because of its possible bearing on his own paper, following it on the program, expressed his gratitude to Mr. Dodge and to the General Electric Company for the opportunity freely afforded him of examining the apparatus used and of studying carefully for some weeks the original records of the observations. He further said:

2 "Mr. Dodge's scheme of keeping the two pressures constant during a run, together with the extremely ingenious graphical method which he has devised for the interpretation of his results, are a notable advance in the technique of throttling calorimetry. Indeed they are among the most original contributions in that field since Professor Peabody's invention of the instrument. I believe that the throttling calorimeter will turn out to be the most sensitive and valuable instrument at our disposal for the investigation of the thermal properties of superheated steam and other vapors. Any future experiments with it ought certainly to be so arranged as to be immediately available for discussion along the lines laid down in this paper. It would, of course, be an advantage, could the arrangement be such as to facilitate also such a discussion as is suggested in the following paper on Total Heats.

3 "Anyone using Mr. Dodge's data should, however, be warned of the necessity of making certain corrections in the low-side temperatures. The usual radiation corrections, which can be determined from tests which he made for that purpose, are uniformly small, seldom exceeding 5 deg. fahr. But even with these corrections his observations afford strong internal evidence of some other as yet unexplained error, by reason of which the low-side temperatures still run consistently low, by from 13 to 15 deg. fahr., the amount of the correction being almost independent of the circumstances of the tests. A detailed discussion of these observations will soon appear in another place.

4 "In conclusion, the straight-line law of Mr. Dodge's first paper is obviously only a rough first approximation to the truth, and *has no theoretical significance whatever*. This fact needs emphasis because the temptation to amuse oneself with thermodynamic manipulations based on a simple law of that sort is very strong. It is true that the lines on Mr. Dodge's Fig. 6 are very nearly straight, but it is also true that a very small curvature in them makes a very great difference in any conclusions that can be drawn from them by the usual thermodynamic processes. And it can be proved conclusively, both from Mr. Dodge's data and in many other ways, that there actually is enough curvature in them to make the contrary assumption wholly misleading."

Dr. Sanford A. Moss. Mr. Dodge's experiments, as well as the throttling experiments of Grindley, Griessmann and Peake are exactly the same as the "Porous Plug" experiments made by physicists. In any such experiment there is throttled expansion, that is, expansion from one pressure to another without doing any work and without radiation or final velocity, the "total heat" being constant. Under such circumstances, the ideal "perfect gas" would remain at constant temperature. Actual gases under most circumstances cool when they so expand, however, and experiments of the type under consideration give the amount of this cooling.

2 I have made considerable study of the law connecting this matter with specific heat, which forms the basis of the present paper. The following statement of the law is equivalent to that given in the paper:

3 Suppose we have a gas at a given pressure P'' and a given temperature A'' and suppose we have throttled expansion to another pressure P' the final temperature being A'. Next suppose we make a

small addition of heat at constant pressure to the substance when at the original condition so as to give a small temperature increment $B'' - A''$ at pressure P''. Suppose from this latter condition we again have throttled expansion to the previous pressure P', the difference between the final temperatures being $B' - A'$.

4 Since the amounts of total heat were constant during the two throttled expansions, the amount of heat which gave the temperature increment $B'' - A''$ at temperature P' is exactly the same as the amount of heat which would give the temperature increment $B' - A'$ at pressure P'. Hence the ratio of values of specific heat at constant pressure at the two points is the inverse of the ratio of temperature increments. It is, of course, to be noted that the two points whose specific heats are thus related are points having the same total heat, that is, on the same line of throttled expansion.

5 I have plotted a diagram like Fig. 6, with all the points of Table 1, for 15 lb. final pressure. I reduced to exactly the same initial pressure all points with neighboring values of initial pressure, by a formula given later. I find that each of the lines for a given initial pressure is a straight line as nearly as can be judged from the rather irregular points. That is to say, when points from all tests with neighboring values of initial pressure are plotted together as in Fig. 6, a straight line well represents them, instead of a curved line as drawn for the single tests in Fig. 6. From these straight lines I have deduced the empirical laws mentioned later. These laws are all based on the results given in Table 1 and may be considered as mathematical expressions well representing the values of this table.

6 As explained later, the conclusion that the lines of Fig. 6 are straight means that the ratio of the two values of specific heat at the initial and final pressures is constant regardless of the temperatures.

7 The plotted points are so irregular that I question the advisability of selecting specific pairs of points and finding variable ratios for them, as Mr. Dodge does in the last two columns of Table 1. It may be that the lines of Fig. 6 are not exactly straight lines. However, I believe a definite conclusion one way or the other cannot be drawn from the experiments given, owing to their irregularity. Hence straight lines, since they represent the points as well as curved ones, are preferable for simplicity.

8 Since straight lines represent the points, it follows that increments of temperature due to addition of same amount of heat at constant pressure at two given pressures have the same ratio regardless of the temperatures involved. That is, values of specific heat at

two given pressures have the same ratio regardless of the temperatures, the proviso being added, however, that the two temperatures have a relation to each other in that they are produced by throttled expansion between the two given pressures. In other words, the ratio of values of specific heats at any two given pressures is constant for any pair of temperatures such that the total heat has the same value. Hence, if lines corresponding to constant values of total heat of superheated steam be drawn on a curve giving values of C_p against temperatures for given pressures, then the ratio of values of C_p for any one pair of pressures will be the same for each pair of points on a constant total heat line.

9 If the results of throttling experiments are plotted as in Fig. 1, the straight line law means that intercepts between any two lines of "constant total heat" on a pair of constant pressure lines, bear the same ratio to each other. That is, referring to Fig. 1,

$$\frac{A'' B''}{A' B'} = \frac{A'' C''}{A' C'}$$

This ratio is, as already discussed, the inverse ratio of values of C_p for the two pressures (at temperatures corresponding to the same total heat). This is also the ratio of the slopes of the straight lines in the figure similar to Fig. 6. It turns out the actual slopes have very closely the ratio,

$$\frac{C_{p2}}{C_{p1}} = \frac{1460 + P_2}{1460 + P_1}$$

The irregularity of the points makes it impossible to say that they give this law exactly. However, they are closely represented by it. It will be interesting to apply this law to other throttling experiments.

10 If the straight lines mentioned are continued by extrapolation, it turns out that they all intersect at 877 deg. fahr. for both ordinate and abscissa. If this is valid, 877 deg. is the "temperature of inversion" for steam for all pressures. For temperatures above this, there is heating during throttled expansion, such as is well known to occur with hydrogen at ordinary temperatures and which prevented its liquefaction. For temperatures below the temperature of inversion, there is cooling such as occurs with most gases, including hydrogen at low temperatures. Cooling during throttled expansion from high pressures, at temperatures below the temperature of inversion, is the usual means of liquefaction of gases.

11 Of course, this exact value of the temperature of inversion may be incorrect. Even if the lines are practically straight for the region of Fig. 6, they may curve enough to give a higher value.

12 The combination of the two mathematical laws mentioned gives as the relation between pressure and temperature (that is, the equation in Fig. 1) of a line of constant total heat or throttled expansion,

$$(877 - t)\ (1460 + P) = \text{constant} = (877 - t_1)\ (1460 + P_1) \qquad (2)$$

Here t is temperature in fahrenheit degrees and P is absolute pressure in pounds per square inch. This relation gives the temperatures at points for which the specific heat ratio (1) holds.

13 Equation (2) is the general law of cooling for porous plug or throttled expansion of steam, and a similar law probably holds with more or less accuracy for other gases. Following are some deductions from this law useful in thermodynamic work or porous plug work such as that of Joule and Kelvin or Rose-Innes.

14 The explicit relation between corresponding drops of pressure in pounds per square inch and temperature in fahrenheit degrees is,

$$t_1 - t = \frac{(P_1 - P)\ (877 - t_1)}{1460 + P}$$

15 The corresponding differential coefficient or variation of temperature with pressure for constant total heat is,

$$\mu = \left(\frac{dt}{dp}\right)_H = \frac{877 - t}{1460 + P}$$

If the temperature is small compared with 877 and if the pressure is small compared with 1460, as is the case when the initial conditions are nearly atmospheric; or if the initial temperature and the final pressure are constant; then the temperature drop is directly proportional to the pressure drop.

16 For all values of temperature, if the pressure is small compared with 1460 or if the final pressure is nearly constant, we have,

$$t_1 - t = K\ (P_1 - P)\ (877 - t_1)$$

17 It is to be remarked that in all of the above discussion, the term "total heat" is used for the total energy which has been put into

a gas to bring it to a given condition. This includes the internal energy actually present as molecular vibration, etc., as well as the energy expended in passing the gas into the region of the given pressure. If we denote total heat by H and internal energy by U and if A is the reciprocal of the mechanical equivalent of heat,

$$H - H_0 = U - U_0 + A\,P\,V - A\,P_0 V_0$$

$$C_p = \left(\frac{dH}{dt}\right)_P$$

Total heat as thus defined is constant during porous plug or throttled expansion.

SPUR GEARING ON HEAVY RAILWAY MOTOR EQUIPMENTS

By Norman Litchfield, Published in The Journal for Mid-November

ABSTRACT OF PAPER

A paper dealing with the breakage of gearing in heavy electric railway service as typified by the equipments of the Interborough Rapid Transit Company, New York, which operates the Elevated and Subway lines. A résumé is given of the methods employed to overcome the breakage, and the strains in the teeth as calculated by the Lewis Formula are shown. Attention is called to the fact that this formula is not entirely applicable on account of the difficulty in maintaining alignment of gear and pinion.

DISCUSSION

Mr. F. V. Henshaw. Gearing for large railway motors offers problems worthy of study by specialists in steel-making and experts in gear-design. Mr. Litchfield contributes some data thereon, derived from what is, considering all conditions, probably the limit of hard service on spur gear drives. Railway motors are limited in dimensions by unchangeable conditions, the fundamentals being largely the same on great railroads and on street car lines. The available space is ample for the small motors (25 to 60 h.p.) required by the latter, gears of liberal dimensions can be provided, and the mechanical problem is confined to reducing wear: in designing motors of 150 to 200 h.p. under the same general limitations, however, the conditions are so far reversed as to require a new departure in gear practice.

2 The situation is illustrated by the following data from three typical cases in practice. The calculated values are in round numbers and based on rated loads at 500 volts. Incidentally, while horse power ratings have much the same meaning in the case of railway motors as when applied to steam boilers, yet they serve the present purpose as a fair measure of the maximum working torque.

TABLE 1 DATA OF TYPICAL RAILWAY MOTORS IN NEW YORK

Specifications	Surface Car	Manhattan		Subway
Horse Power	40	125		200
R. P. M. Armature	500	560		500
Torque, inch-pounds	5000	14000		25000
Torque, pounds at p.d	1770	4670		6250
Feet per minute " "	740	880		1045
Pitch	3	3	2.5	2.5
No. Pinion Teeth	17	18	16	20.
Face, Gear and Pinion	5	5	5	5.25
Sq. in. Tooth at p.d	2.62	2.62	3.14	3.3

3 A committee of the A. S. & I. Railway Association has recommended the following gear sizes as general standards:

For motors of	45 to 100 h.p.,	3	diametral pitch,	5	in.	face
" " "	100 " 250 "	2.5	" "	5.25	"	"

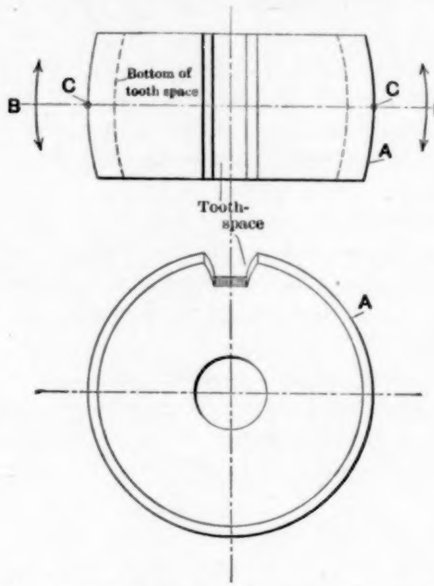
4 In short, owing to various limiting conditions, there is no kind of proportion between the motor capacities and the gear dimensions in use. The conditions for large motors are even more severe than indicated above as they actually run on higher voltages and consequently at higher speeds than stated; this fact accounts for the higher pinion velocity given by Mr. Litchfield. By the use of interpole motors with forced ventilation still greater motor capacity can be obtained with the same available motor space so that cases may arise in which gear-strength will be the governing factor in large motors.

5 As to the solution of the problem, three suggestions are quoted in Par. 10, and the author offers a further suggestion in Fig. 6 for overcoming the inevitable lack of shaft alignment. The first suggestion is the use of a coarser pitch and any possible misconception of the situation may be cleared away by the broad statement that any improvement involving reduced gear-ratios or increased gear-diameters may as well be ruled out. The highest practicable gear-ratios are none too high for many cases, there is little or nothing to spare in reducing either the number of teeth or the diameters of pinions, gear cases have no clearance to spare, and we cannot help the situation by using larger wheels. It appears then that the solution of the problem of satisfactory gearing for large railway motors must be looked for in the material and form.

Mr. John Thomson. The accompanying diagram (Fig. 1) indicates a solution of the problem in Par. 20 of this paper, that can

readily be accomplished by the use of ordinary gear-cutters. This consists in curving the periphery of both the pinion and its gear, each blank thus becoming a spherical zone, as *A*. Then when forming the leaves of the gear, either the blank or the cutter is to be swung in the arc of a circle corresponding to that of the peripheral surface, say as *B*, with *C* as the axis.

2 The consequence of this would be that in first starting the operation of such a pair of gears, there would be point-contact only at their centers; but as these wear away the area of contact would



PROPOSED METHOD FOR SECURING CONTACT BETWEEN TEETH AND A CENTRAL PLANE

increase. When using involute contours, I do not see that this would affect the action; detrimentally on the contrary as new surfaces were brought into contact the pitch-ratio would be maintained constant. Moreover, incorrect initial alignment, or that resulting from wear and tear in practice, would seem to be met automatically.

3 I have not tried this method and the idea is an off-hand shot; but if it is worth the while, anyone is welcome to use it.

MR. J. KISSICK, JR. There is, to the writer's mind, a remarkable similarity between the problems of the elevated and of the subway

service. The greater part of the problem will be solved for both when the prolongation of the life of the tooth is accomplished: in this respect the two problems are similar.

2 On the elevated division the introduction of the wrought steel rim gear and the adoption of a $2\frac{1}{2}$-diametral pitch has solved the question of breakage, thus leaving the brief wearing-life of the teeth as the final obstacle to be overcome.

3 Experience on the elevated road naturally caused the adoption of similar gearing equipment in the subway, although with different results. Here, we understand, the trouble seems to be breakage in the pinion teeth, and to a lesser extent in the gear teeth. Since it has been deemed unsafe to run a pinion which has worn to $\frac{3}{16}$ in. at the top, it is obvious that any remedy which will prolong the wearing-life of the teeth will also prevent the large number of failures due to a high fiber stress induced in the worn tooth by the accelerating torque.

4 Mr. Litchfield has considered the problem, it seems to the writer, merely as one of a cantilever beam, the tooth being loaded at the crest and supported at the root. Being a case of simple flexure, one needs but to vary one or all of three things to enable the tooth to support a given load:

a Increase the depth of the beam, which, however, would be at the expense of the meshing tooth, although by increasing the angle of obliquity, as in the 20-deg. involute stub tooth, the tooth may be thickened to a certain extent at the root.

b Increase the elastic limit, which has been accomplished by the substitution of wrought rim gears.

c Decrease the moment by shortening the lever arm of the load, which has been brought about by the use of stub teeth.

5 Were the problem one which concerned the supporting of a static load, the above remedies would be sufficient; but being also a gearing proposition, other factors enter in that have to be met. There is no doubt that the use of steel having an elastic limit of 90,000 lb. per sq. in. and the use of 20 deg. involute stub-teeth, while they have not eliminated all the trouble, have at least caused it to appear in mitigated form. The difficulty seems to be that these remedies do not go far enough, and do not treat the problem from a gearing standpoint as well as from that of a loaded beam.

6 An extended experience covering railroad work and crane serv-

ice (in the latter case breakages of ordinary cast steel gears and pinions causing a menacing situation) has brought to light several factors not ordinarily considered as affecting the strength of gear teeth.

7 The first of these is resistance to wear. This, of course, needs no explanation, for it can be very easily realized that with a thick tooth there is less chance of failure than with a tooth which has been worn thin.

8 The second is permanency of form and resistance to deformation, the two being more or less related. Ordinary conditions of construction and service preclude any possibility of theoretical engagements of involute teeth, and we have the substitution of sliding for rolling motion. The result, naturally, is abrasive action, which ordinary steel gears are unable to withstand; the tooth curve below the pitch line becomes flattened and slightly undercut, the amount depending upon the smallness of the pinion and consequent obliquity of action. The loss of the involute curve and the additional back lash destroy the intended action of the tooth, and instead of one set of teeth taking the load before another lays it down, we have one set carrying the whole load and transferring it bodily to the succeeding set at some point near the pitch point at the rate of some 1800 blows per minute. The effect of this hammering is commonly called "wear," but nothing could be further from the truth. No ordinary steel could stand up under these blows, and consequently we find the metal flowing towards the edges and top of the tooth, depositing small slivers eventually in the gear case. An examination of the accumulation at the bottom of the tooth would show these slivers, and also a deposit somewhat similar to anvil scale.

9 The fact is that the ordinary steel gear does not wear, in the true sense of the word. Its inability to keep its shape under the severe hammering exaggerates the fault as time goes on, and premature failure is the final result. The use of stub-teeth does not help this condition any, for even the most perfectly cut tooth of this design transmits power with a very jerky motion.

10 A new steel pinion placed in service with a half-worn gear does not exert a corrective effect on the gear tooth, but instead, falls a prey to the evil tendencies of the gear. The problem therefore might be stated as follows:

- *a* A $2\frac{1}{2}$-diametral pitch 20-deg. involute stub-tooth affords a very strong tooth design.
- *b* Metal to withstand 90,000-lb. fiber stress at the elastic limit is essential.

c The material comprising the tooth, in addition to sustaining the above fiber stress, (1) should be proof against wear by abrasion; (2) should offer maximum resistance to deformation, thus retaining as long as possible its original form; (3) should not be brittle, or fracture under repeated blows.

11 The latter clause really means the substitution of a special steel. Manganese steel fulfills all the requirements, being tough and extremely hard, and having an extremely high elastic limit.

12 Its wearing properties are well known, for it is the only metal which can be used successfully in crushing-machinery, and in fact in all places where abrasion takes place. Its resistance to deformation is one of its peculiar characteristics, limiting its use in ordinary commercial work, but making it particularly valuable for the service under discussion. It has been demonstrated in comparative tests that the more prominent this characteristic is, the longer the life of the gear. Where the gear was made very soft, the size of the burr on the edge of the tooth was, to a certain extent, inversely proportional to the length of service.

13 The early considerations affecting the designs of manganese gears were naturally foundry ones, which required an even and comparatively thin metal section. Experience has overcome the casting difficulties, however, and the manufacturers have been able to turn their attention to the character of the work required of their product.

14 It is the writer's firm conviction that the use of manganese steel gears, properly designed, will solve this particular problem.

Dr. George William Sargent. Much stress has rightly been laid upon the desirability of stronger material that the motor equipment of the subway may obtain relief from its present gear and pinion troubles. Resistance to wear seems to me of the greatest importance in modifying the stresses to which the tooth of the gear or pinion is subject. Wear on the teeth develops play, with a consequent change in the character of the stresses. The load which was delivered statically is now delivered dynamically and must be reckoned upon as a blow. Energy expended in the form of blows tending to rupture a piece of steel is much more destructive than the same energy gradually applied. Hence the utmost resistance to wear is a desideratum as much to be sought as increased strength; and do not the nature of the breaks described in Par. 17 prove this to be the case?

2 Fig. 3 is illustrative of all the failures. "It will be noticed that points *a*, *b*, *c*, *d*, and *e* gave way successively until at last the loose piece was caught and ripped out." (Par. 17.) Such a break produces a detailed fracture not inconsistent with the statement that, as wear increased the play, thereby causing greater severity of the blows, the tooth began to rupture, ruptured in a small degree, then in a larger degree, and at last completely. To prevent absolute failure, resistance to wear is of the utmost importance, and this leads to a consideration of hardness as a quality determinative of ability to withstand abrasion or wear.

3 It is well known that as hardness in steel increases, brittleness develops, hence there is a hardness limit beyond which, on account of the product's lack of toughness, it is not safe to go. This limit varies with each kind of steel. The carbon steels have the lowest values for hardness and toughness at this limit, thus a 45,000 lb. per sq. in. elastic limit carbon steel has a hardness of 191 (Brinell scale), a 90,000 lb. per sq. in. elastic limit carbon steel a hardness of 286, and 140,000 lb. per sq. in. elastic limit alloy steel a hardness of 400. In the first instance the values are about the maximum obtainable from an ordinary carbon steel; in the second from an extraordinary carbon steel; and in the last instance the values might be raised a little before the point of safety would be exceeded. Although it is obvious which steel is superior, consideration of the lives of the gears made from the respective steels may make it yet clearer.

4 Assuming, although it is not by any means the case, that a body twice as hard as another will resist wear twice as long, a gear made of the extraordinary steel, with its elastic limit double that of ordinary steel and its hardness increased 50 per cent, should have its life doubled on account of its increased strength and made half as long again on account of its increased resistance to wear, making a total increase in the longevity of the gear of two and one-half times. With the alloy steel gear, reasoning after the same manner, the life will be increased over that of the ordinary steel gear twice by the hardness and three times by the strength, a total of five times. These figures are too low for the facts, but they show relative values and the importance of the matter of hardness. It is stated that in practice it is impossible to maintain perfect alignment between gear and pinion. Too great hardness is therefore to be avoided, since the points of contact might be made too few, bringing excessive strain upon these few points, with the resultant rupture.

5 Failures of machines in service usually result from one or more

general causes, two of which are, improper design and failure to select the proper constructive material. These are usually due to a lack of appreciation of the conditions of service, although they may be due to the development of changed conditions of service. The design may be correct and the material of construction right and either improper methods of handling the material or imperfect manufacture of the material or both may yet bring about failure. Judging from the fact that the factor of safety is but 1.1, and a change in the design raises this factor 1.6, both improper design and the failure to select proper constructive material seem to be responsible for the conditions. That this is the case is further indicated by the character of the fracture described in Par. 17 as typical of all the failures.

6 It is in just such instances that the alloy steels, with their greater possibilities, find their usefulness. Increased dimensions are not permitted, altered designs effect but a partial relief, and a harder and stronger constructive material must be used. True, these alloy steels are more expensive in their first cost, but really less expensive in the final reckoning. Where life hangs in the balance, cost should be not even a secondary consideration.

Prof. F. De R. Furman. Mr. Litchfield's suggestion of giving the tooth a double-curve form can hardly solve his problem, for it will still leave the entire load of the teeth concentrated on point contact under all conditions. If the contact point should move to one side, due to improper alignment of the wheel shafts, the pressure would come at a reduced and therefore weakened section.

2 Using the data given in this paper, I have laid out the proportions of teeth obtained by standard rotary cutters (according to proportions given in Kent) for both 14½-deg. and 20-deg. pressure angles, which the author states were used, and then compared these teeth with others of special form which figure out to be much stronger. The comparative forms are shown in Fig. 1 and 2, in which teeth of standard proportions are shown entirely in section, the proposed form with shaded edges. In Fig. 1 the 14½-deg. pressure angle is used, and in Fig. 2 the 20-deg. angle.

3 I have made the comparison by considering the tooth as a beam fixed at one end, and have drawn within each tooth a parabola having its vertex at the middle of the crest and its two branches tangent to the outline at or near the root. Then applying the formula $hP = \frac{1}{6} Fbt^2$, in which h = height of tooth, P = pressure on tooth at the tip, F = working stress in tooth, b = breadth of tooth (taken as 3 in.),

and t = thickness of tooth, we have for F = 39,400 lb. (the figure given by Mr. Litchfield), P = 7700 lb. for the standard tooth having a 14½-deg. angle. For the proposed tooth with the same angle, taking P = 7700 as above, we find F, the stress at the root = 23,600 lb., against 39,400 for the standard tooth, an increase in strength of 67 per cent in favor of the special form of tooth.

4 For the tooth with a 20-deg. angle we find for the same value of P (7700 lb.) that the stress in the standard tooth is 35,600 lb., against 25,600 lb. for the proposed tooth, an increase of 39 per cent. While

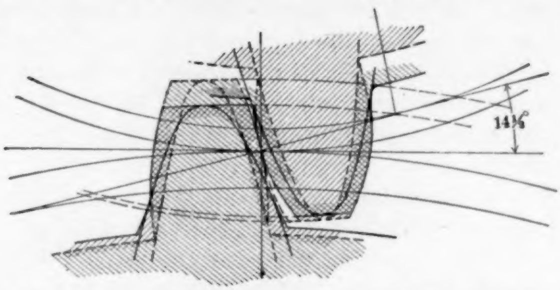


Fig. 1 Comparison of Tooth Forms 14½ Deg. Angle

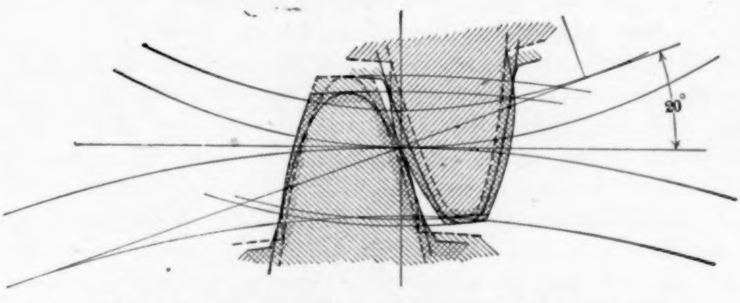


Fig. 2 Comparison of Tooth Forms 20 Deg. Angle

the 20-deg. tooth shows up the stronger for the standard form, the 14½-deg. tooth is the stronger in the proposed form. This is due principally to the decreased length of tooth for a given path of action, made possible by the 14½-deg. angle, and also to the specially formed fillet which starts from a point well up from the root circle.

5 The amount of action in the 14½-deg. proposed tooth is about 1.5, practically the same as for the standard form, assuming that that part of the standard tooth which is above the interference line is not in action.

6 The large fillets at the roots of the proposed teeth were found by tracing the path of the corner of one tooth on the plane of the other wheel and then placing the tooth outline within this path. The parabola was then drawn tangent to the tooth outline. In making the above computations the weaker tooth on the smaller wheel was used in each case.

7 It will be noticed that the addenda for the two teeth are not equal, a point referred to in my discussion of Mr. Flanders' paper.

The Author. It was not the purpose of the writer to attempt to approach this subject from the standpoint of the gearing-expert but merely from that of the user of a considerable amount of gearing material on an exceedingly intense service, and to lay before the Society those encountered difficulties which might be of interest, together with the efforts made to overcome them.

2 While it is impossible to take up the discussion in detail, in the limited time allowed for review of the various points, correction should be made of a seeming misapprehension on the part of one writer who states: "There is no doubt that the use of steel having an elastic limit of 90,000 lb. per square inch and the use of 20-deg. involute stub-teeth, while they have not eliminated all the trouble, have at least caused it to appear in mitigated form."

3 At the time of the preparation of the paper practically none of the gearing under our observation was of the stub-tooth, high elastic-limit type, and while since then a large number have been installed, the length of service has obviously been too short for judgment of the value of the improvement.

4 Much stress is laid by others on the value of alloy-steels, and it should be stated therefore that the attractiveness of several of them has been such as to lead to a thorough investigation of their merits, which is still under way. These investigations include actual service tests in quantities large enough to give a fair indication of results to be expected from a general adoption of the particular class of steel in question. Until these data, coupled with those yet to be found for the treated carbon-steel, are obtained, it would be improper to discard the carbon-steel and adopt the alloy.

5 Aside from the question of the tooth form the situation may be thus briefly summarized:

> The carbon-steel has failed and data must be obtained from service of the resistance to failure of (1) Specially treated carbon-steel; (2) Alloy steel.

If one shows a manifest superiority over the other, then that should be adopted, but if they compare favorably, the problem will then have changed from one of failure to one of wear, and we will at the same time be in possession of the information necessary to determine this second phase of the question of gearing on heavy railway equipments.

INTERCHANGEABLE INVOLUTE GEAR TOOTH SYSTEMS

By Ralph E. Flanders, Published in The Journal for Mid-November

ABSTRACT OF PAPER

This paper gives diagrams showing the effect of varying the pressure angle and addendum on the various practical qualities of gearing, such as interference, number of teeth in continuous action, side pressure on bearings, strength, efficiency, durability, smoothness of action, permanency of form, etc. After comparing typical examples of interchangeable gear systems in these particulars, the author concludes that a new standard for heavy, slow speed gearing is advisable.

DISCUSSION

Mr. Wilfred Lewis. I think Mr. Flanders has presented a very illuminating and complete analysis of the conditions governing the intelligent use of interchangeable involute gearing from a 12-toothed pinion to a rack. It appears from this that the so-called standard 14½ deg. system is really no system at all, but rather an evolution by a sort of trimming process from the errors of the past.

2 About thirty years ago when I first began to study the subject, the only system of gearing that stood in much favor with machine tool builders was the cycloidal in which the describing circle was made one-half the diameter of a 12-toothed pinion, thus giving it radial flanks. At that time I was called upon to investigate a cutter forming machine in which the faces and flanks of the cutter were approximated by circular arcs. I soon came to the conclusion that the true form of such cutters could not be approximated by circular arcs near enough to give satisfactory results, and I worked out a scheme whereby the machine was modified to form the faces and flanks each by two tangent arcs instead of one. Some improvement in action resulted from this change, but the gearing produced was still noisy, and this improvement was followed by the very radical one of building a cutter forming machine in which the shapes produced were actually rolled.

For some time thereafter Wm. Sellers & Company, with whom I was connected, continued to use cylindrical gearing made by cutters of the true shape, but the well known objection to this form of tooth began to be felt, and possibly twenty-five years ago my attention was turned to the advantages of an involute system. The involute systems in use at that time were the ones here described as standard, having 14½ deg. obliquity, and another recommended by Willis having an obliquity of 15 deg. Neither of these satisfied the requirements of an interchangeable system, and with some hesitation I recommended a 20 deg. system, which was adopted by Wm. Sellers & Company and has worked to their satisfaction ever since. I did not at that time have quite the courage of my convictions that the obliquity should be 22½ deg. or one-fourth of a right angle. Possibly, however, the obliquity of 20 deg. may still be justified, by reducing the addendum from a value of one to some fraction thereof, but I would not undertake at this time to say which of the two methods I would prefer. In a general way I think Mr. Flanders has raised a very important question, and one which should be taken up by the Society.

3 I brought up the same question nine years ago before the Engineers' Club of Philadelphia, and said at that time that a committee ought to be appointed to investigate and report on an interchangeable system of gearing. We have an interchangeable system of screw threads, of which everybody knows the advantage, and there is no reason why we should not have a standard system of gearing, so that any gear of a given pitch will run with any other gear of the same pitch.

4 I would therefore propose as the author suggests, that this subject be referred to a Committee of the Society to investigate and to report upon the adoption of a standard system of involute gearing. The paper considers gears from a 12-tooth pinion to a rack only, and that is as far as I would go with such a system. If internal gears are employed, they would necessarily have to be more or less special. I think Mr. Flanders has covered the subject in a very clear and concise way. He has done a great deal towards the solution of the question by properly stating it, and when anything is properly stated, it is half solved. I therefore make this a motion. (The motion was considered later in the discussion.—Editor.)

Mr. Luther D. Burlingame. I can easily believe that my friend, the author of this paper, found the solution of the interchangeable gear tooth problem a task far greater than he had anticipated.

While many writers appear to reach some rather definite conclusions, I believe that the usual experience of investigators along these lines has been voiced by Mr. Fred J. Miller in the American Machinist, "I think that it is the experience of most men that the more they have studied on the matter of tooth-gearing, the more clearly it has appeared to them that they would never be able to believe anything in regard to it." I take this opportunity to express my appreciation of the able and fair-minded way in which Mr. Flanders has dealt with so difficult a subject, and one that can be viewed from so many points.

2 Mr. Flanders bases his data for the 14½-deg. pressure-angle entirely on a form of tooth which is a true involute for its entire length. As such a tooth is not made or recommended by any manufacturers, as far as I know, and as what is made would give a radically different showing in the comparative tables of the paper, this seems to be setting up a "straw man" to knock over, rather than "tackling" the real thing. It can be said in extenuation that the author used the data at hand; that the data in commercial use were not available is not surprising, as they have been derived by manufacturers through years of experience and at great expense; furthermore, the giving out of such data, even were it good policy, most probably would never result in the production of good gears, much less of interchangeable gears. The experience of the company with which I am connected is that the old saying, "a little knowledge is a dangerous thing," is most applicable to the science and practice of gearing.

3 In illustrating the difference between the theoretical and the commercial tooth of 14½-deg. pressure-angle, reference is made to Par. 7, 10 to 19 inclusive, 33, 37, and 38, and to Fig. 5, 8, 12, 13, and 14, as well as to the table accompanying Par. 45, Comparison of Selected Examples of Involute Gear Tooth Systems. These deal to a greater or lesser extent with the question of length of contact of the engaging teeth and with the question of the number of teeth in continuous action. To show the radical difference between the results obtained in the paper and based on the use of the uncorrected involute form of tooth with 14½-deg. pressure-angle, inadvertently called by the author the Brown and Sharpe system, and the results actually obtained with cutters made by that company, I would refer to Fig. 1 to 3. As a matter of fact there are two teeth driving for half of the time, or on a basis of Mr. Flanders' table referred to above, 1.5 teeth in continuous action. As his table gives 0.98 of a tooth in continuous

action, the real Brown and Sharpe tooth shows a gain of more than 50 per cent above the results tabulated as the Brown and Sharpe standard. If all of the statements in paragraphs and figures above referred to should be modified to this extent, they would give a real comparison instead of a hypothetical one. Thus referring to Fig. 14, Case 1, the diagram would be radically modified for the Brown and Sharpe system where at least two teeth are driving for half of the time, the commercial tooth approaching nearer to what is shown in Case 5 of this same figure.

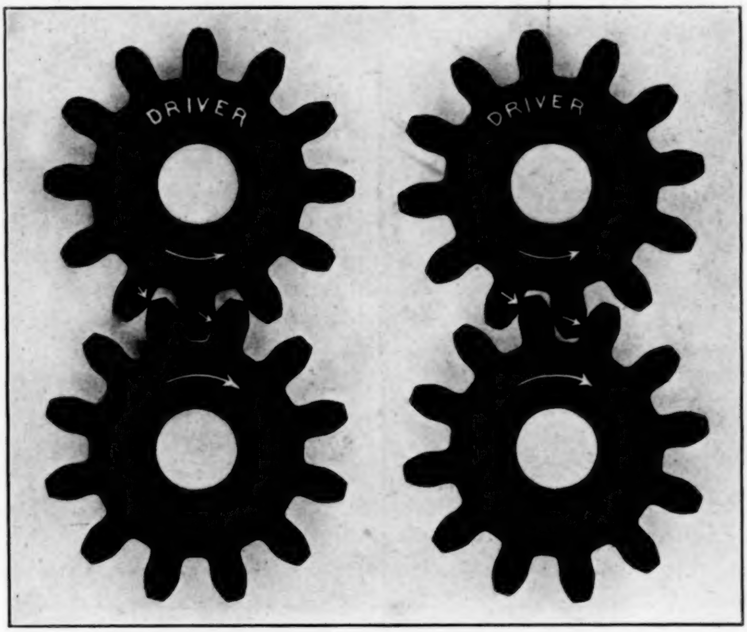


Fig. 1 Fig. 2

Twelve-Tooth Pinions B. & S. Interchangeable 14½-deg. System

4 The teeth of the gears cut with Brown and Sharpe cutters are slightly eased off at their points so as to come gradually into engagement, thus insuring smooth and quiet running. Experience has shown that such a modification is not only important but essential, and in any system, no matter what the pressure angle and height of addendum, I believe the teeth should be so modified. When such a modification is made it becomes a mere academic contention whether the corrected part is modified from a true involute or something else.

I understand there are methods covered by patents for accomplishing a rounding and easing off of the points of the teeth when they are formed by the generating process.[1]

5 The impression is given by the author that there are difficulties in the way of using the generating process which limit it and that the devising of a new system of gearing will make its use more satisfactory. This seems like a case where, as the mountain will not come to Mohammed, Mohammed must go to the mountain. To consider

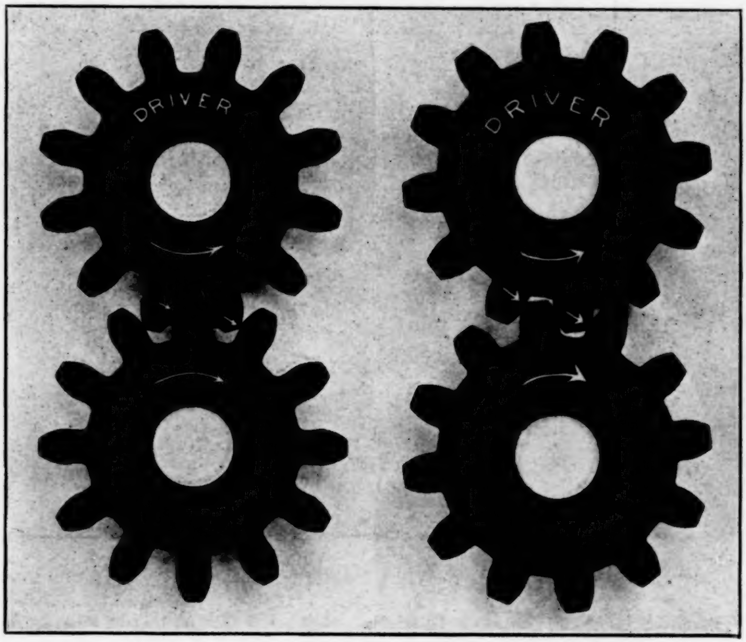


Fig. 3
Brown and Sharpe Interchangeable 14½-deg. System

Fig. 4
Pressure Angle 20 deg. Depth 0.7 Pitch

the adoption of one system for gears made by the generating process and another for those made by the use of formed cutters, would be like adopting the metric sytem for calculations and the English system for the use of the workman, considering each as adapted respectively to these particular uses.

6 In the Brown and Sharpe practice, to meet their own needs and those of their customers, cutters and gears are made with pressure

[1] Bilgram Patents No. 749,606 and No. 749,683.

angles varying from 4 to 28 deg., and with a height of tooth ranging in both directions far outside of the limits discussed in Mr. Flanders' paper. It is an everyday matter to produce gears and cutters with such variations, and they can be made at least with equal facility, as compared with standard shapes, by the use of formed cutters. Such cutters, as made by Brown and Sharpe for 20 and 22½-deg. pressure-angle, are for each angle interchangeable.

7 Mr. Flanders has given little consideration to the question of *backlash*, in fact has not mentioned it in Par. 45, where he sums up the objections to using a greater pressure-angle and less addendum. In many classes of work this becomes an important consideration and any system tending to increase backlash is objectionable. It is a common practice in making special gears for printing presses and other places where backlash must be reduced to a minimum, especially where the center distance of the gears must vary appreciably, to make the pressure-angle as low as 4 deg. and to increase the addendum to a greater length than standard. In any case, the greater the pressure-angle the more backlash there will be with a given inaccuracy in center distance, a given variation in setting the cutter or tool when producing the gear, or a given amount of wear on the teeth.

8 There are indeed special cases where a greater pressure angle than 14½ deg. is sufficiently desirable for various reasons to off-set the objections made. The Brown and Sharpe Company use on their machines gears with a greater pressure-angle, when the conditions make it seem desirable, and they make such gears and cutters for customers whenever called for. These, however, are invariably made with a correction for smooth and quiet running, even when the pressure angle and height of tooth are such that theoretically this would not be required.

The Reinecker generating machine has provision for easing off the points of the gears to prevent noise.

9 Frank Burgess of the Boston Gear Works says[1] regarding the height of tooth, "A long tooth usually gives a better movement than a short stubby one. With the shorter tooth, the pitch is proportionately greater for its depth and there is a tendency to jump from one to the other, especially for a pinion with less than 20 teeth, and this tendency results in noise. The noise in gearing is undoubtedly the result of shocks, jumps and vibration caused by teeth coming into and going out of action."

[1] American Machinist, June 27, 1907, p. 935.

10 While the use of shorter teeth or a finer pitch would theoretically make some saving in time of cutting, we do not find such a saving appreciable.

11 I would ask the author why the possibilities of inaccuracy mentioned in Par. 30 as applying to the use of formed cutters are not also present in the generating method, instead of being limited as stated in Par. 31. It would seem to me that most of these possibilities of inaccuracy would be equally present in both systems.

12 With all our experience at the Brown and Sharpe works, our experts feel that the subject of gearing is full of pitfalls, and the more experience we have, the less we feel like dogmatizing or appearing as authorities. While all theories should be examined with an open mind, I believe that a spirit of conservatism should govern their investigation until they are proved by practical experience to be correct.

13 I do not understand that the author suggests an abandonment of the present system, but states rather that the "discussion points clearly to the wisdom of an alternative gear tooth standard of shorter addendum and increased pressure angle." The fact is that modified forms of teeth, of a sort approved by the author and many other forms also, are now used in special cases where they prove to be better. Every manufacturer of gear-cutters knows this to be true, though at the present time, with the large demand for gears in automobile construction, there may be an emphasis upon a tooth of a certain form. What this emphasis will be in future years is uncertain. As likely as not another pressure-angle and another form of tooth may be insisted upon. Is it not better to leave these matters to the manufacturers of gear-cutters and gear-cutting machines who are willing to give the public what they want rather than attempt to formulate a system which the experience of the next few years may possibly render of little value?

Mr. D. F. Nisbet. I agree fully with the author's conclusion that an alternative standard of involute teeth for heavy, slow-speed gearing is desirable; particularly so for such gearing as rolling-mill pinions, where, owing to the limited diameter and tremendous shocks, it is necessary to use few teeth of large pitch, and obtain a comparatively smooth action by making the gears either double helical or of the staggered tooth type.

2 Smoothness of action, as understood and defined by builders of the finer classes of machinery, is not essential for rolling-mill work;

perhaps continuity of action would better express the desideratum for this class of work. All that is necessary is a degree of smoothness of action that will not leave traces of "harsh gearing" on the finished product. This applies with particular force to plate mills and sheet mills, and in a lesser degree to all types.

MR. CHARLES WALLACE HUNT. It should be borne in mind that this paper treats of a system of interchangeable gears; that is, all of the pinions and gears of each series having the same addendum. This commercial requirement eliminates the consideration of theoretically perfect forms of conjugating teeth for special conditions and confines the discussion to what Mr. Wilfred Lewis designates as the "best compromise between conflicting conditions."

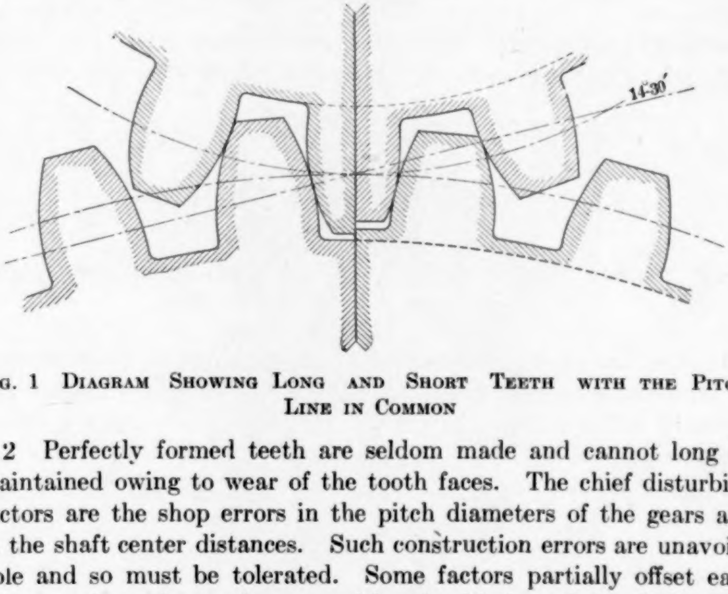


FIG. 1 DIAGRAM SHOWING LONG AND SHORT TEETH WITH THE PITCH-LINE IN COMMON

2 Perfectly formed teeth are seldom made and cannot long be maintained owing to wear of the tooth faces. The chief disturbing factors are the shop errors in the pitch diameters of the gears and in the shaft center distances. Such construction errors are unavoidable and so must be tolerated. Some factors partially offset each other, such as the tension caused by the friction of the sliding surfaces of the tooth after passing the center which weakens the tooth. The fillet at the root of the tooth strengthens it. Usually the favorable and the adverse factors partially balance each other, but the working formulae must provide for the unexpected which, as Prof. Sweet reminds us, frequently happens. After considering and valuing the various factors that affect the problem, the designer will decide on proportions that, in his judgment, will give the best general result in practice.

3 In the gear work of the C. W. Hunt Company it is assumed that

The angle of action is 14½ deg.

The whole load is carried on one tooth.

A pinion with less than 19 teeth should not willingly be used.

If the pinion is strong enough its conjugating wheel is also.

A table is quicker and safer to use than a formula.

A table based on fibre stress is preferable to one based on the names of materials.

4 The total length of teeth frequently referred to, expressed as a percentage of the circular pitch, is

Rankine	0.75
Sir Wm. Fairbairn	0.70
Brown and Sharpe	0.687
Molesworth	0.66
Coleman Sellers	0.65
Hunt	0.55

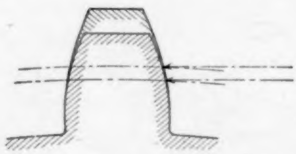


Fig. 2 Diagram Showing a Short Tooth Superimposed on a Long Tooth with the Root-lines in Common

5 After ten years' use in a wide range of machinery, I believe that for commercial machinery the following proportions for cut teeth are especially suitable ones. If they are not ideal proportions the difference is slight. The face of the tooth is involute in form, the angle of action is 14½ deg., and the length of the tooth is 0.55 of the circular pitch.

HUNT TOOTH FORMULA

Addendum	0.25 of the circular pitch.
Dedendum	0.25 " " " "
Clearance	0.05 " " " "
Length of tooth	0.55 " " " "

TABLE 1 WORKING LOADS

Spur Gears, one Inch Face

Diametral pitch	1	1¼	1½	2	2½	3	3½	4	5	6	7	8
Circular pitch	3.14	2.51	2.1	1.57	1.25	1.04	.897	.785	.628	.524	.449	.393
Pounds on the pitch line	2250	1800	1500	1100	900	700	600	565	450	375	300	282

6 Table 1 gives the working strength of a spur tooth, having parallel flanks. The tabular load applied on the pitch line produces a fibre stress at the root of the tooth of 5000 lb. per square inch. If applied at the pitch line plus one-half of the addendum then the fibre stress will be 7100 lb. If at the extreme end of the tooth 9200 lb. per square inch.

7 For a safety factor of $3\frac{1}{2}$ when the load is applied at the end of the tooth:

Cast iron (16,000 lb. ultimate strength), use a working load of one-half the tabular number.

Steel (64,000 lb. ultimate strength), use twice the tabular number.

GEAR CUTTERS

8 The initial stresses in the casting frequently cause a distortion when the gear is finished. A spoked gear-wheel blank may be turned

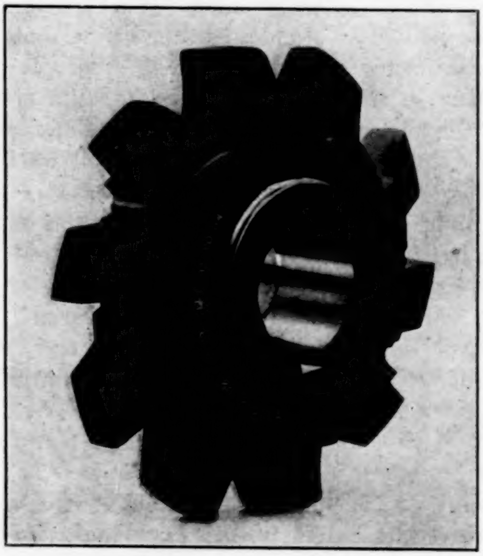


Fig. 3 Perspective View of the Cutter

perfectly round in a lathe, but after the stocking cutter has been used the points of the teeth are seldom in a true circle. The rim between the spokes either bulges out, or falls in, depending on the initial stresses in the metal. This is frequently a material amount

and is objectionable as it alters the pitch line and the tooth spacing, and also injuriously affects the clearance.

9 It has been found advantageous to have tooth cutters (Fig. 4) with a wing on each side which will trim the length of the teeth to the standard size. With these cutters a gear made of metal without a hard scale can be cut without sizing the blanks in a lathe.

10 Cutters for teeth proportioned by the above formulae are made for any one desiring them, by Brown and Sharpe, Gould and Eberhardt, or any other gear-cutter maker.

Mr. Oberlin Smith. The suggestion has been made that a committee be appointed to standardize gear teeth, with the assumption that there would be one standard. Another has said that it is best to leave it to the makers, as they know the needs of their customers; the result of this would be forty, or fifty or even a hundred standards, as we have now. It would seem wise to standardize the teeth, even if there should have to be several kinds. We need to do this almost as much as we needed to standardize the screw thread. It would be useful to have some standard which most people would follow, and undoubtedly in this way a standard practice could be established. The committee would consider the different conditions, as for instance, those met in heavy rolling-mill work, and choose different pressure-angles adapted to each case.

2 If a committee is appointed to consider this matter, I hope they will take out fractions and make the angle of obliquity 15 deg. rather than 14.5 deg. Suppose they found 15 deg. good for a certain class of gears, and 20 deg. good for another class, and 25 deg. good for another class, why could we not have two or three standards, under certain names, so that people could use them to comply with the conditions?

3 If we do have a committee on gearing, their investigations should go further than merely determining shape. It would be well to instruct them to find out all they can about strength of gears as well. The matter of strength is very important, and it is a thing which we do not know very much about. Many of us go by the Lewis formula, by which, it seems to me, the required strength increases too rapidly with increase in speed. But there are conditions of impact and percussion which cause the teeth to vibrate and make them break at high speed. We do not know what these conditions are, and that is what the committee should find out.

4 One thing desirable, in interchangeable gearing, is to be

able to use fewer teeth in pinions. Some one said that nine is the lowest number desirable. The Brown & Sharpe Mfg. Co. give twelve as the least practical number to get a theoretically good wheel, and very likely that is so. I have used pinions with four, for some 15 or 20 years, without perceptible wear where the pressures were not great but the speeds were very high, the small pinion being the driver. I have some lathes which have five-tooth pinions doing the driving; they are still in use and are a perfect success. Of course, it is probable that the angular motion is somewhat irregular with such a small pinion.

5 It is often practicable to use a pinion with from five to ten teeth, as a driving but not as a driven gear. It is well to avoid "strain" of several gears by using only two, having the pinion as small and gear as large as possible. It is often good practice to cut such a pinion in the solid shaft. We are building some heavy presses, of 1000 tons capacity, where the pinions are more than a foot in diameter, forged integral with the shaft, one at each end. A common practice in rolling-mills is to use two helical pinions separately keyed on, with pitch reversed to avoid end-thrust and ends abutting. I recently built a mill for a foreign government, with two 16-tooth pinions forged solid with their shafts where two projecting collars served as blanks for the pinions, and by the simple expedient of separating them, left room for a Brown & Sharpe cutter to run through, into the space between.

Prof. W. Rautenstrauch. It seems as if there were sufficient interestin the principles of machine manufacture to justify the formation of a section to consider papers of this kind and draw fire along this line. I have had the matter in mind for some time, and have talked with other members, and it seems to meet with approval. I therefore propose that we meet this afternoon to consider the formation of such a section, and draw up a letter petitioning the council to form such a section immediately. (The meeting was held and a committee appointed to formulate plans for a machine shop section to be submitted to the council and reported at an adjourned meeting at the time of the spring meeting of the Society.—Editor.)

Mr. Charles H. Logue. After reading Mr. Flanders' paper, it would be useless for me to enter a discussion as to the need of a universally accepted standard for gear teeth, especially for large gears. It is now common practice for all users and makers of large

gears, to correct the teeth for interference, either by shortening the addendum or increasing the angle of obliquity as compared with the 14½ deg. system, or both. This procedure, of course, entails additional expense in manufacture, as special cutters and formers must be made for each gear. Again, each manufacturer, and in many cases the individual user has his own ideas as to how this correction for interference should be made, and also as to the amount.

2 I wish to express my approval of the purpose of Mr. Flanders' paper, namely: the appointment of a special committee of the Society to recommend an interchangeable gear-tooth standard, and also of the action at the last meeting of the Society whereby a recommendation was made to the Council that such a committee be appointed. The present situation is little short of chaotic, and the present "so-called" standard was most aptly characterized by Mr. Wilfred Lewis as "no standard at all." The work before this committee is mountainous, for this problem is probably more complex than any other individual problem in machine design and construction that could be presented. The difficulty of the problem is also a measure of the benefit that will come to the machine-building industry if a standard is recommended and brought into general use.

3 It is apparent that there will be a great saving in the adoption of a rational, universal system, as well as an improvement in tooth-action, the extent of which is entirely unappreciated by gear users. Only a comparatively small part of the gear-tooth surface is in actual use, and the rest of this surface is a detriment rather than a help to the tooth-action. In addition to this detriment, the correction as sometimes made produces surfaces that give an irregular impulse, or series of impulses, to the driven gear by the driver. Thus the teeth of the driven gear have instantaneous accelerations and retardations and lack a regular motion. This, of course, tends to destroy the gears rapidly. I believe that this condition prevails to a greater or less extent, in all involute gears as ordinarily made.

PROPOSED SHORT TOOTH STANDARD

4 In order to set before the Society information as to existing gear-tooth systems, I propose to describe a short-toothed, 20-deg. standard that I began to use some three years ago. Since that time I have advocated this system for all gears heavier than one-diametral pitch, and in many cases for gears of finer pitches. I now see no reason why this system is not universally adaptable. It follows closely the Fellows system as worked out for fine pitches, except that

the addendum bears a definite relation to the circular pitch. This relation is expressed by the equation,

$$addendum = 0.25\ circular\ pitch$$

5 This quantity was derived from experience and from a study of the tooth parts of the Fellows system. In the Fellows system the addendum varies from 0.264 of the circular pitch to 0.226 of the circular pitch. These factors for the common combination pitches are shown in the following tabulation:

For 2/2½	pitch,	addendum—	0.255	of circular	pitch.
" 2½/3	"	"	0.264	"	"
" 3/4	"	"	0.240	"	"
" 4/5	"	"	0.255	"	"
" 5/7	"	"	0.228	"	"
" 7/9	"	"	0.247	"	"
" 9/10	"	"	0.254	"	"
" 10/12	"	"	0.264	"	"
" 12/14	"	"	0.226	"	"
" 14/18	"	"	0.250	"	"

6 The factor 0.25 is, therefore, a rough mean of the factors shown in the table, and is also a convenient quantity to use in computing gear-tooth parts. This latter advantage is especially true as the thickness of the teeth at the pitch-line, added to the pitch diameter, gives the outside diameter, and this relationship is of convenience when measuring gears in which the pitch-diameter and pitch are unknown. In many cases the only dimensions received by the gear manufacturer are the outside diameter and number of teeth. While this feature should not be an influence in determining the length of addendum to be used, it is a convenient point to keep in mind. For diametral-pitch the addendum is found by dividing 0.7854 by the pitch. This is a well-known factor; is equal to $\frac{\pi}{4}$; is in general use in many engineering formulas, and is easily memorized.

7 The tooth parts for circular pitch are:

Addendum	0.25 ×	cir.	pitch,	instead of	0.3183
Dedundum	0.30 "	"	"	"	0.3683
Working depth	0.50 "	"	"	"	0.6366
Whole depth	0.55 "	"	"	"	0.6866
Clearance	0.05 "	"	"	same as now used	

8 The tooth parts of diametral pitch are:

Addendum	$\frac{0.7854}{P}$	instead of	$\frac{1}{P}$
Dedundum	$\frac{0.9424}{P}$	"	$\frac{1.57}{P}$
Working depth	$\frac{1.5708}{P}$	"	$\frac{2}{P}$
Whole depth	$\frac{1.7278}{P}$	"	$\frac{2.157}{P}$
Clearance	$\frac{0.157}{P}$	same as now used.	

9 In addition to the length of the addendum the other important element of the gear-tooth, to be determined, is the obliquity. While the increase in journal-friction with the pressure-angle is not as great as is generally supposed, yet this angle should be kept as small as is consistent for the purpose intended; that is, to obtain an interchangeable involute system without correcting the tooth-outlines. In the system which I am describing 20 deg. is the angle of obliquity. Thus the two essential elements of the system are, the addendum equal to 0.25 of the circular pitch and the pressure-angle of 20 deg. These correspond very closely with Case 4, as set forth in Par. 44 and 45 of Mr. Flanders' paper. He characterizes Case 4 as a system having an angle of obliquity of 20 deg. and an addendum height equal to $\frac{0.8}{P}$. For circular pitch the addendum would equal 0.8 or 0.2513 of the circular pitch. That is, Case 4 is essentially the system which I am describing.

10 Referring to Par. 45 of Mr. Flanders' paper, from his tabulation captioned, Comparison of Selected Examples of Involute Gear Tooth Systems, I have transcribed the factors:

Smallest pinion in series, to avoid interference	14
Maximum number of teeth without correction for interference with 12-tooth gear	36
Maximum and minimum number of teeth in continuous contact	1.38 1.18
Proportion of side pressure on bearing to tangential pressure	1.064
Strength-factor of rack	0.543
Strength-factor of 12-tooth gear	0.354
Comparative loss of work from friction	552
Comparative durability	1.44

11 He states that the smallest pinion in the series that can be used and avoid interference is one with 14 teeth. The lowest theoretical number of teeth for such a pinion engaging a rack is 13.4. I believe, however, that 13 teeth can be used without correction as the error is less than ½ a tooth, therefore in order to use a 12-toothed pinion, which is today considered a *desideratum*, the only correction necessary is for this pinion alone. In some practice, street railway use in particular, 14 is the smallest number of teeth which is ever used; 12 and 13 are always avoided.

12 In order to use an uncorrected 12-tooth pinion in this instance, without changing the addendum, it would be necessary to increase the angle according to Mr. Flanders' formula, to 21 deg. 13 min. If it is necessary to include the 12-tooth pinion in the interchangeable system, I would prefer to see the angle increased for the reason which I have indicated above. However, I would strongly recommend for consideration an angle of 20 deg. and the addendum as given, and thus consider a 12 and perhaps a 13-toothed pinion as special. These would then be in the same situation as are 10 and 11-toothed pinions today.

13 It will be argued that the short-tooth standard permits of a smaller number of teeth in contact. While this may be true, in some cases, the superiority of this contact has been demonstrated by Mr. Flanders. However, it should be noted that, owing to the increased strength of the tooth that has been shortened and has a wider angle of obliquity, the pitch may be reduced accordingly to give the same number of teeth, or more, in contact, as are in contact in similar gears having the usual length of addendum and a pressure angle of 14½ deg.

14 As a matter of interest it must be pointed out that this system is now in use for the gears of the subway trains in New York, and to the best of my knowledge it is superior in service to the standard-toothed gears that have been discarded. The special 20-deg. involute stub-tooth system described by Mr. Litchfield in his paper on Spur-Gearing on Heavy Railway Motor Equipment, is the same as this short-tooth 20-deg. standard that I have proposed for consideration.

Mr. E. R. Fellows. The conclusions arrived at by the author, as given in table accompanying Par. 45, do not entirely coincide with the writer's experience. The reason is that the form of 14½-deg. tooth considered is virtually a short tooth of this angle, and has theoretically, in these deductions, some of the advantages of the stub-tooth.

The forms selected by the author for the standard or 14½-deg. tooth are those which he infers are produced by a set of formed milling-cutters designed to cut an interchangeable set of gears. As, however, the exact form of these cutters is more or less of a trade secret, he has evidently been obliged to make his own deductions upon some points, such as the exact modification for interference and the angle of the flank below the base line. A little variation in these points makes a considerable difference in theoretical efficiency.

2 The stub-tooth so-called, which is a short tooth of 20-deg., being very little modified for interference, conforms very closely to the theoretical. The standard forms selected give n, the number of teeth in contact, as 0.98. It has been the general experience that gears having a line of action as short as this do not run satisfactorily; they are noisy. And this is the test by which gearing is approved or condemned; a difference of one or two per cent in efficiency being of no importance whatever. An examination of any satisfactory set of gearing of standard form will show that most of the gears bear nearly if not quite to the point. This means that the value of n is ordinarily 1.5 to 1.75, and while the running qualities are satisfactory the efficiency is lower than if the value of n were 0.98.

3 This being the case, it is safe to assume that either the form of standard cutters ordinarily used is not the one discussed by the author, or that gearing, after running a short time, changes from wear sufficiently to give a longer line of action. The author evidently had this condition in mind, as his formula for strength assumes that the stress is applied at the extreme point of the teeth, notwithstanding the fact that according to his first deductions, this would be impossible. We will consider the changes necessary to give to n the value of general practice.

4 In case of two 12-tooth pinions shown in Fig. 2 and 3, the author assumes that interference begins as soon as the theoretical action ends. This is not the case. The path traced by the point D, Fig. 3, beyond the so-called interference point, is an epitrochoid, which so nearly conforms to the theoretical involute, that if the flank of the tooth below the base line be undercut 2½ deg., D will rub the entire distance *ab* of the point of the meeting tooth. The radial flank causes what little interference there may be.

5 The design of cutters for an interchangeable set is a matter of compromise. Strength demands as broad a flank as is possible for the pinion; efficiency demands a short line of action; quiet running requires a longer one. The latter being the point by which the user

tests his gears, it is safe to say that it receives the most consideration. A little compromise would obviate even the undercutting of the flank.

6 Theoretically, in the interchangeable set two pinions of 12 teeth should mesh together. Practice does not demand this, such a case being extremely rare. This combination would give a very low efficiency. If this is imperative, as in the case of spur gear differential of the automobile, gears of greater angle are almost invariably used. It is safe to say that the meshing of two 15 or 16-tooth pinions would fulfill all practical requirements, and anyone who has rolled two standard pinions of 12 teeth together, in the condition which they leave the cutter, will decide that in practice all gears are not absolutely interchangeable.

7 The writer's experience has been largely with generated gears, but from this experience, he would say, that the best practice in gearing of the standard form demands a length of action giving about $n = 1.5$, and that this requirement is met. This value of n would materially change most of the diagrams given, particularly Fig. 12, comparing the lost work, and Fig. 13, comparing durability. If the modification for interference of the standard 12-tooth pinion, instead of being 0.4 of the addendum, be 0.0, and if this modification up to about 30 teeth be a very immaterial amount, the friction or lost work of the average train of gears will be considerably more than that given by the author.

8 In this connection the term "corrected for interference," used by the author and by other authorities on gearing, is open to criticism. As the involute curve is theoretically correct, and after "correction" is only *correct* in the sense that it will mesh with another form which is not in itself theoretically correct, the term "modified" more nearly describes the case.

9 The writer would emphasize the statement in Par. 37 regarding durability. The fact that wear, in the case of the stub-tooth, is distributed over a much greater surface, is a strong point in its favor. This is more marked in the case of the pinion, where most of the wear usually takes place. The result is, that the form of the worn-out stub-tooth is practically that of an involute curve.

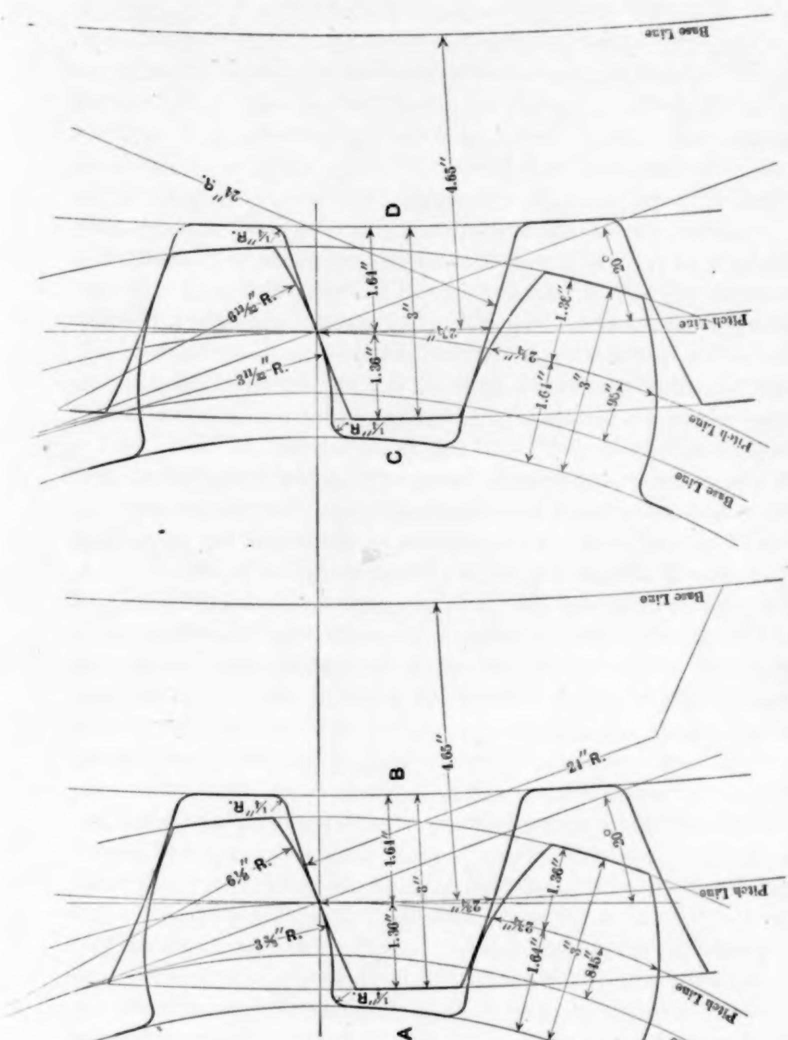
10 The "handicap" of "one form of generating process" is not as serious as might be inferred from Par. 39. As the modification for interference is very simply taken care of by the design of the cutter, it is possible to vary this modification automatically, and to give the teeth of each diameter of gear just the required amount and no more.

11 One important point not touched by the author, is the effect

of what may be termed "shop efficiency" upon the comparative running qualities of the standard and the stub-tooth systems. A shop efficiency of 100 per cent is possible only under laboratory conditions. 90 per cent represents what is probably first-class commercial conditions, and 70 per cent is more common. The running qualities of both types of gears would probably be very nearly the same under laboratory conditions, but under practical ones the balance is in favor of the stub tooth and increasingly so as the shop efficiency decreases.

Mr. Tho. Fawcus. We find the Brown and Sharpe system sufficient in every respect up to 1 P. For large gears we generally use the 20-deg. involute, with a depth of 0.55 of the circular pitch, or about 0.8 S. A layout for some 5½ in. pitch gears recently made is shown in Fig. 1.

2 The gear manufacturer is usually in the unfortunate position of having to assume responsibility for gears designed by others for work of which he is not informed. If the gears fail, it is blamed on the only detail left to his discretion; namely, the kind of iron or steel or other material used. A recent case where material was blamed for being "too soft," upon investigation showed gears put to a service calling for nearly four times the strength by Lewis' formula. For these reasons I think the question of durability has not received sufficient attention. Tooth forms do not trouble gear manufacturers much; it is easy to make conjugate involute teeth, and, for heavy gears at least, the short tooth is preferable. But we need to know more about the durability of certain combinations of metals at high and low speeds. This is most important in gears under heavy load and at low speed where the material must be such as will not crush on the line of contact; also in worm gearing, which is in a class by itself with regard to the materials of which it should be made. The usual practice of making the worms of steel and the wheels of cast-iron is very bad. But if the gear-maker offers any objection, he is confronted with the fact that the combination has proved satisfactory in some particular case. That results are most varied and even contradictory is true; one pair of worm gears succeeding where a perfectly similar pair will fail. This is sometimes partly explained by excessive friction caused by tight bearings in new machines, or shaft centers too close together, or insufficient lubrication. The cutting action once started is difficult to stop and usually results in the destruction of the wheel.



Special 20-deg. Teeth

Pinion A, 16 Teeth; Pitch, 5½-in.; Face, 18-in.; Pitch-Diameter, 28-in.; Radius Base Circle, 13.155. in.
Gear B, 88 Teeth; Pitch, 5½-in.; Face, 18-in.; Pitch-Diameter, 154-in.; Radius Base Circle, 72.35 in.
Pinion C, 18 Teeth; Pitch, 5½-in.; Face, 18-in.; Pitch-Diameter, 31½-in.; Radius Base Circle, 14.8 in.
Gear D, 88 Teeth; Pitch, 5½-in.; Face, 18-in.; Pitch-Diameter, 154-in.; Radius Base Circle, 72.35 in.

3 This is, I know, departing somewhat from the subject under discussion, but I mention it because it is a very real difficulty with which engineers and manufacturers have to contend.

Prof. F. DeR. Furman. At the end of Par. 12 the author states: "In Fig. 7, it will be seen, a similar increase of addendum makes no change in the number of teeth in contact, as the action is still limited by points C and D." In Par. 15 it is stated: "When $\alpha = 20$ deg. and $S = 1.0$, the amount of contact between any two gears of the same series from 12 teeth to a rack is constant at about 1.4" and also "that if in any series there is interference in the case of two pinions having the smallest number of teeth allowed by the series, the amount of action obtained in that case is constant for any other case throughout the whole series, up to that of two racks meshing with each other. As far as the author knows, this condition has never before been noticed." Also in Par. 15 we find: "The 14½-deg. standard series of whatever height of addendum gives less than continuous action, being about 0.987."

2 All the above statements must rest on the assumption that there is no contact between the tooth surfaces beyond the interference line, in which case there is a fair portion at the end of the tooth that could be cut off without interfering in any way with the running. If a set of interchangeable wheels were made with conjugate extensions beyond the interference line none of the statements quoted would hold. Since the extension cannot be involute, and must be of some other form, that form might as well be the simplest one, which is the epicycloidal. And further if we give the epicyloidal form to a fair portion of the face, and flank, and leave only a portion of the tooth near the pitch circle, of involute form, we may as well abandon the involute system entirely for interchangeable gearing, and adopt the epicycloidal system instead for interchangeable work.[1] Then in the endeavor to perfect gearing we could at least work towards theoretical conditions, instead of using the approximate methods referred to in Par. 32 for forming the extensions to the involute beyond the interference line. These extensions I understand are not in practice true conjugate curves and therefore must allow, theoretically at least, the follower to slow up while the driver

[1] To meet the difficulty involved in cutting epicycloidal teeth at the pitch circle, we could adopt a combination outline for interchangeable teeth in which the tooth form is involute for a short distance on either side of the pitch line and epicycloidal for the remainder of the face and the flank.

continues at its uniform velocity, the result being a blow as the teeth come into contact.

3 The author shows that in a system having an angle of $22\frac{1}{2}$ deg. or more, and where $S = 0.8$, there is no interference, and it follows that the teeth may be correctly made of the involute form all the way to their tips. But even here smoothness of action will not be obtained so long as the teeth are cut with standard rotary cutters in which each one must cut a certain range of wheels. Take for example the No. 3 cutter cutting 35 to 54 teeth, it would follow that the cutter must be formed to suit the involute of the smallest wheel and therefore would cut away too much of the face of all wheels having 36 to 54 teeth. This means that there will be correct driving contact only at or near the line of centers, and as the driving pair pass through receding action the follower slows up, thus allowing the next tooth of the uniformly moving driver to come into violent action with impact and noise, instead of easy contact with the follower tooth.

4 In Par. 24, the author states that "In order to get the proper form of fillet on the 12-tooth pinion, the rack tooth was lengthened by an amount equal to the clearance, and the corner of the extended tooth was rounded with a radius equal to $\frac{2}{3}$ of the clearance." I would like to ask why the author goes to the trouble of lengthening the tooth and then rounding it off instead of simply finding the curve traced by the corner of the rack tooth on the revolving plane of the pinion and then placing the fillet curve within the one thus found?

5 From the present paper it would seem that there would always be difficulty, if not impossibility, in producing a series of smooth-running interchangeable gear wheels, all the way from a 12-tooth pinion to a rack on the involute system. If a series of intermediate sized changeable involute wheels were desired the problem would appear to be an easier one.

6 The most difficult part of the problem for interchangeable involute wheels lies in the fact that the face or addendum must be the same throughout the series. It is quite different, however, for an independent pair of wheels which are always to run together. Here the addenda may be made unequal to great advantage, thereby avoiding in most cases the trouble due to interference, and giving a receding action greater than the approaching by any desired amount. This involves the making of a special cutter for each wheel, but where a large number of wheels are to be duplicated the extra expense for cutters would be more than offset by the smoothness due to correc theoretical action.

7 An example of the use of teeth having unequal addenda will be given in a discussion of Mr. Litchfield's paper to be presented this morning. In that illustration it will be shown that a very strong tooth may be obtained with a 14½-deg. pressure angle because: (1) the necessary amount of action may be obtained with this angle by a relatively short tooth; (2) a specially-formed large fillet may be placed at the root.

8 From the above, and from deductions from Mr. Flanders' paper, it would appear that if we give to high-class gearing the special consideration which each case deserves, theory is pointing to the use of a small pressure-angle for two wheels that are to run always together, and to a large angle for a series of interchangeable wheels, if the involute system is used.

Mr. A. L. DeLeeuw. The main point which has been made perfectly clear to me is that there is some difference of opinion on this subject, which is of some significance as showing that many of us have reached a point where we are striving for something different, supposing it to be better.

2 The motion by Mr. Lewis is to the effect that the council be requested to appoint a committee to consider a standard system of gearing. I am inclined to think this motion should be broadened by making it read "standard systems" instead of "a standard system." Not necessarily that we wish more than one system, but that it would leave the committee which investigates the matter free to consider it from all possible angles. I will therefore second the motion of Mr. Lewis and ask him kindly to assent to my suggestion to broaden the terminology slightly so as to include any kind of system or systems which may be proposed.

Mr. Lewis accepted this suggestion, with the proviso that the resolution should be confined to involute interchangeable gearing; and also a suggestion by Mr. E. H. Neff that it be put in the form of a recommendation to the Council that they take action upon its provisions. The motion which is expressed by the following, was then put and unanimously carried.

2 Resolved: That the Council be asked to appoint a committee to investigate the subject of interchangeable involute gearing and recommend a standard, or standards, if found desirable.

The Author. Referring to Par. 2 of Mr. Burlingame's discussion. it should be noted that I did *not* base my data on a form of tooth

which is a true involute for its whole length. In fact, a main point of the investigation was to show how little of the outline (about one-third) could, in the standard form, be involute. This is one of the grounds for criticism of the present form of tooth, which thus relinquishes, to a great extent, the advantage which the involute curve gives, of perfect action at varying center-distances. Besides the small length of theoretical curve possible makes the form of tooth indeterminate, except by empirical methods. I followed an old suggestion in making the indeterminate portions of cycloidal form; this was done only in order to obtain a reasonable working-basis from which to start the investigation, and was made necessary by the lack of definite information from the proprietors of the present system. I apologize for calling this hypothetical system the "Brown & Sharpe" system. This was an inadvertence; proper correction will be made before the paper is published in a permanent form.

2 In Par. 3, Mr. Burlingame objects to basing the calculations for the number of teeth in contact, etc., on the small amount of theoretical action possible with this hypothetical tooth form. (And this in spite of his previous assertion that the calculations were based on a tooth of true involute form for their whole length.) This procedure is justifiable, since, in a partially involute form of tooth, only the involute portion remains in action if the center-distances (as must be expected to happen in practice) are slightly greater than called for theoretically.

3 It is true, as intimated by Mr. Burlingame in Par. 7, that the smaller the pressure angle, the smaller the backlash when the gears are slightly separated. This should have been included as one of the advantages of the present system for such uses as change-gears, printing-press work, etc.

4 I do not wish to be put in the position of discounting the complexity of the problem of interchangeable gearing. This question is indeed one of some difficulty. I do feel, however, that a new solution may be safely sought, through the ability and experience at the command of this Society. The fact that the Brown & Sharpe standard gives excellent results for a wide field of work, even when the whole range from 12 teeth to the rack is covered by a set of 8, or at the most, 15 cutters, would indicate that the refinements hinted at are more imaginary than real; and the fact that two very different systems of interchangeable gearing (those mentioned by Messrs. Lewis and Hunt) have been tried out by exacting and competent engineers over a long period of years and over a wide range of applica-

tion, to the entire satisfaction of the users, would indicate that departures from the old form can be made without fear of disastrous results. Furthermore, there is no practical engineering problem which cannot be solved by the combined application of technical training, perseverance, and common sense.

5 Mr. Fellows (Par. 1) makes the criticism that a longer theoretical action would have been obtained if the 12-tooth pinion had been considered as having the involute extend out to the points of the teeth, and if the flanks of the mating teeth and been undercut, when necessary, to allow this. It is true that more action might be obtained by a system so designed; this action would be truly involute in large gears, but in the case of small pinions it would amount, practically, to a rocking of the face of the tooth about the base of the involute on the mating tooth. This action is purely fortuitous, and is not susceptible of analysis. Besides, it undercuts the flanks of small pinions, leaving a distinct shoulder at the base line. This shoulder does not appear in teeth shaped by standard-formed cutters, where the involute merges smoothly into what appears (on the 12-tooth pinion) to be practically a radial flank. Thus no escape is left from the conclusion that teeth shaped by standard-formed cutters depart at their points very materially from the true involute form. On the other hand, if my memory serves me, a 12-tooth pinion generated by Mr. Fellows' process shows a distinct shoulder at the base circle, so it is very likely that the involute in that case extends nearly or quite to the points of the teeth.

6 Professor Furman calls attention to the fact that the cycloidal system avoids all the corrections and modifications necessary with the involute system; and he suggests that its claims be considered in place of the involute for use in a standard system. It is true that the cycloidal form shows a marked advantage from the standpoint of pure kinematics; but, as Mr. F. J. Miller has pointed out, natural evolution, in free competition with the involute form, has resulted in the practical elimination of the cycloidal system. One of the disadvantages of the latter is the difficulty of obtaining sufficient side clearance for formed cutters; another is the difficulty of generating the curves as compared with the involute. For a general purpose system of interchangeable gears, the cycloidal form is "out of the running."

7 Professor Furman, in Par. 2, asks why I used the method described for generating the flanks and fillets of the teeth used in finding the strength-factors. The most convenient way to study a

standard system theoretically is to consider it as generated from a standard rack. This has its counterpart in actual practice in the action of the gear-hobbing machine. To obtain, then, a rationally practical form of tooth, it was considered to be generated from a hob. In making a hob, I would round the corners as described, to obviate a rough or stepped generation of the fillet; this procedure also makes the fillet as large as it can be, and still be safe from interference with a sharp-cornered rack tooth.

8 His suggestion in Par. 6 that much better results can be obtained by departing from the regulation proportions, is true. I have given much study to this matter. Many cases are found where the added expense of special cutters is warranted. This use of special gearing is becoming more common than many designers realize, especially in such work as printing-presses, metal-planers, etc., where the best possible results are required. I wish to make the suggestion that all such special gearing be stamped with some recognized symbol to show that it is special. This would avoid costly errors later, when the inevitable repairs have to be made, since it is often impossible to tell by inspection, or by any ordinary means of measurement, whether or not a gear is of standard form. All this is aside from the question at issue, however.

9 Mr. Logue's discussion expresses the viewpoint of the engineer who has specialized in the design and manufacture of gearing, and very logically and forcibly describes present conditions in the field of heavy work. These conditions now appear to be well on the road to remedy, thanks to the action of the members and the Council of this Society.

THE SLIPPING POINT OF ROLLED BOILER TUBE JOINTS

By Profs. O. P. Hood and G. L. Christensen, Published in The Journal for Mid-October

ABSTRACT OF PAPER

The object of this paper is to supply data regarding the behavior of joints made by the familiar process of rolling boiler tubes into containing holes. Attention is called to the fact that the stress at which the tube slips is as important as the ultimate strength of the joint. The results of experiments on tube holes of various forms and with various degrees of rolling are shown by plotted curves, and a simple method is indicated whereby the slipping point of the usual joint may be raised so high as to bring the full elastic strength of the tube into use.

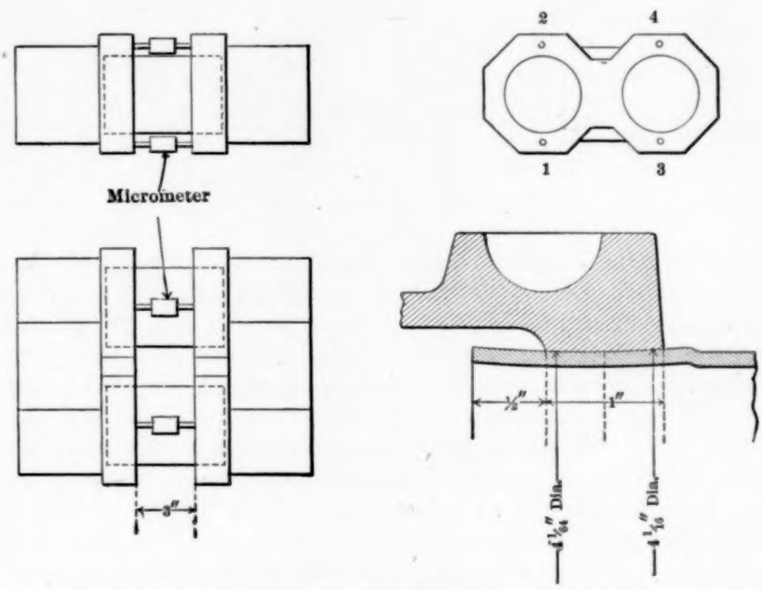
DISCUSSION

Mr. J. C. Parker. Professors Hood and Christensen have shown a new phase of the subject of expanded joints. The straight non-beaded joint with about ½ in. seat has come into extensive use in water-tube boiler practice. These tests make it evident that there is considerable movement with joints of this character, especially in boilers where the tubes are not free to expand independently, and the shorter the joint the greater will be the leakage.

2 Boiler insurance companies have lately been demanding that the tube ends project ½ in. past the seat and be flared about ⅛ in. Comparing tests No. 2 and No. 4 the flaring does not appear to have strengthened the joint in any way. This agrees with our tests, which did not show a noticeable increase in holding power from flaring over the ordinary straight rolled joint which has a slight flare from the taper mandrel. Some projection past the seat, however, is essential, and ½ in. appears to be about right.

3 When Professor Hood drew our attention to this subject we made a number of tests of 4-in. tube joints with water pressure.

Pairs of boxes were connected by two nipples 6 in. long and the slip measured by a micrometer between the backs of the boxes set in center punch marks. The accompanying table gives the results of one of the tests. The micrometer was set up in the centers so it would not spin freely and by holding it lightly between the fingers a less slip could be detected than could be measured. In no case did the micrometer indicate any spring or stretch or any movement whatever until the slipping point was reached, when a little jump would occur each time and all the measurements would be found to be increased.



METHOD OF MEASURING SLIP IN TESTS UPON BOILER TUBE JOINTS

4 The slip invariably began around a pressure of 1150 lb. per sq. in. and further slip occurred generally with each 100 lb. added to the pressure. A pinhole in a handhole cover developed at a pressure of 2100 lb. per sq. in. and was calked. At 2660 lb. one of the tube joints began to leak.

5 Short joints are likely to leak from slipping, but a tube well rolled in a long joint acts like a tight-fitting piston which can slip without leaking. The joint shown has proven in practice to have ample holding power and rarely shows any leakage with ordinary rolling.

TESTS OF 4-IN. ROLLED BOILER TUBE JOINTS

Slipping Point. Pressure, Pounds per square inch.	Slip in Inches				
	No. 1	No. 2	No. 3	No. 4	Average
1150	.0017	.0011	.0012	.0002	.00105
1270	.0045	.0008	.0090	.0062	.0051
1510	.0029	.0035	.0003	.0001	.0017
1650	.0024	.0023	.0008	.0012	.0017
1760	.0013	.0005	.0010	.0004	.0008
1870	.0005	.0005	.0006	.0005	.0005
1950	.0031	.0037	.0004	.0014	.00215
2060	.0006	.0012	.0016	.0010	.0011
2150	.0024	.0012	.0036	.0034	.00265
2220	.0008	.0010	.0007	.0007	.0008
2340	.0017	.0017	.0008	.0010	.0013
2450	.0023	.0024	.0011	.0012	.00175
2525	.0022	.0022	.0009	.0012	.0016
2660	.0004	.0015	.0045	.0068	.0033
	.0268	.0236	.0265	.0253	.0255

Prof. C. H. Benjamin. Professors Hood and Christensen are to be congratulated on the success of their experiments in a comparatively new field. Their work seems to offer the first reliable data on the holding power of expanded boiler tubes, a subject on which there has always been considerable discussion and speculation.

2 The use of this method of fastening tubes in modern sectional boilers, where the integrity of the boiler depends upon the holding power of the tube, has rendered the question of even more importance than formerly. In the water-tube boiler of the Babcock & Wilcox type, where the front headers connect with the steam drums above and the rear ones with the mud drum below, the safety of the structure depends entirely upon the permanence of the union between an expanded tube and the header or drum.

3 It seems to me there are one or two points brought out in these experiments which are particularly interesting. First, the slight slipping of the joint at a comparatively low pressure and the fact that this slipping is not particularly affected by increased rolling or by flaring the ends of the tubes. Although it may be desirable to raise this slipping point somewhat, as a matter of economy, it is undoubtedly safer to have it stay where it is. Since slipping will usually be evidenced by leakage, we have here a warning of failure at a comparatively good factor of safety. If the slipping pressure is brought too near the breaking pressure, the first evidence of weakness might be complete rupture of the joint.

4 Second, that flaring does not seem to have the important effect as a safeguard which has sometimes been claimed. There has been considerable argument in the past, especially between boiler makers and insurance companies, as to the necessity or advisability of flaring. It is evident from the diagrams, as in Fig. 4, that initial slipping will not be prevented by flaring to any extent and, as in the case of Curves 2 and 4, the flaring would have but little effect on the ultimate strength. All of the diagrams and figures would seem to show that medium or hard rolling has more effect in raising the ultimate strength of the joint than any flaring of the tube. I have always believed this to be the case, because rolling produces the friction between shell and tube to prevent initial slipping and it also produces a shoulder or abutment outside the shell which comes into play a little later (See Fig. 1). The flaring, on the other hand, does not come into play until there has been considerable slipping of the tube. The claims made by some authorities that flaring increases the strength 300 per cent are evidently erroneous. The difference is probably due to better rolling.

5 The margin of safety in expanded tube joints can be illustrated by considering a 4 in. boiler tube of 10 gauge, expanded in a plate of $\frac{5}{8}$ in. thick. The net area of the tube will be 1.627 sq. in. and if we call its tensile strength 60,000 lb. per sq. in. the ultimate strength of the tube in tension will be 97,620 lb. Allowing for friction between tube and shell 750 lb. per sq. in., it would require about 5900 lb. to slip the joint and from 8000 to 12,000 lb. to pull it out. With a pressure of 150 lb. per sq. in. inside the shell, the pressure tending to push out the tube would be 1884 lb. We thus have a factor of safety of over 3 as regards initial slipping and as regards ultimate failure, a factor of safety of from 4 to 6. A comparison of these figures, however, with those representing the strength of the tube, will show that we are very far from having 100 per cent joint.

6 The facts brought out with regard to the serrating of the surfaces are very interesting. I think this should be done with care since it is better to have the point where leakage will show considerably below the ultimate strength of the joint.

Mr. E. D. Meier. I think the tests including even the least result show that there is ample security against danger from slipping of the tubes. The authors have shown by their careful experiments that the opinion prevalent among boiler manufacturers that it is not necessary to flare the tubes if you roll them in tight, and further, that it is not necessary to use excessive rolling, is well grounded.

2 A simpler method than that suggested for preventing leaky tubes is to keep oil out of the boiler. I have never known a case of leakage around tubes that had been well rolled and properly set that could not be traced to oil. If in rolling in the tubes there is the slightest film of oil left on the tube or sheet there will be the same trouble.

Mr. M. W. Sewall. I have been interested in comparing the slipping point referred to in the paper and the leakage point as shown in the tests on tubes, made by The Babcock & Wilcox Company.

2 The tests referred to were made by subjecting the tubes to hydrostatic pressure after they had been expanded into properly prepared flanges, having ½ in. wide seats. The fluid pressures at the leakage points are given below, whereas the tables in the paper give the force tending to drive the tubes out of their seats. With very light and improper expanding on 3¼ in. tubes, leakage occurred in one case at 300 lb. pressure, in another at 500 lb. per sq. in. Reduced to the stress tending to force the tubes out of their seats this becomes 2490 and 4150 lb. respectively.

3 When expanded properly the leakage points for 3¼ in. tubes were 800 to 1450 lb. per sq. in. These become when reduced to the stress tending to force the tubes out of their seats 6640 and 12,035 lb. respectively. The end pressures at the leaking point are somewhat comparable with those shown by the curves in Fig. 2 of the paper. It will be noticed, however, that the values obtained with the light and improper expanding, namely 2490 and 4150, are somewhat higher than those showing the slipping point in the curves A, B and C. The figures for the end pressure in the cases of properly expanded tubes, which show 6640 and 12,035 lb. respectively, are higher than the initial slipping point shown in curves 22 and 19. In fact, there is no case in Fig. 1 to Fig. 5 showing an initial slip as high as 12,000 lb. There are four curves, three in Fig. 2 and one in Fig. 3, showing slips of 0.01 in. with a stress higher than 12,000 lb.; all the others are considerably lower.

4 I would like to ask whether, where a decided thickening of the tubes is shown in the tube sheet in the engravings of the paper, there was an error in making the cuts. Apparently not, because it is repeated several times. I would like to know how the thickening is accomplished if the illustrations correctly represent the conditions.

The Authors. In order that comparisons can be made with tests upon other tubes it is necessary to note that the radial pressure and

therefore the friction which can be developed by the rolling process varies directly with the thickness and inversely as the diameter of the tube.

2 Also a tube of larger diameter and the same normal pressure, having a greater circumference and bearing area, would offer a greater resistance to slip in proportion to the increase in diameter.

3 Also that if the force be applied as an internal fluid pressure this is added to the normal pressure produced by rolling and should increase the holding power.

4 Mr. Sewall's direct experiments on the leakage point as distinguished from the slipping point are of great interest and value, and it is hoped that the complete experiments may be made available to all. The figures which he has given should be modified to make them comparable with the three inch tube tests.

5 The $3\frac{1}{4}$ inch tubes used by the Babcock & Wilcox Co., are presumably standard 11 gage tubes which are 10 per cent thicker than the 12 gage 3-inch tubes. The tube diameter is also $8\frac{1}{3}$ per cent greater; therefore the normal pressure probably developed in the $3\frac{1}{4}$-inch tube when rolled would be 110 per cent $\div$ 108.3 per cent $= 101.5$ per cent of that in a 3-inch tube. The area of the seat would also be $8\frac{1}{3}$ per cent more than in the 3-inch tube so that we should expect $101.5 \times 108.3 = 110$ per cent more resistance to slipping if the stress were applied in the same way.

6 With the stress applied as an internal fluid pressure of 300 to 500 lb. per square inch and assuming a coefficient of friction of 0.3 then 450 to 750 lb. of the resistance found in the B & W tests with light rolling was due to the internal test pressure and the remaining 2040 to 4400 lb. was 110 per cent of what would be expected with the thinner and smaller 3-inch tubes.

7 This makes the comparable figures 1855 to 4000 lb. and quite within the range of the curves shown in Fig. 2. It would appear that had the slip of these tubes been measured it would have been found that leakage occurred with a slip of less than 1/100 of an inch, for the resistance of the 3-inch tube at 1/100 in. slip was 6000 lb. Referring to the properly expanded tubes cited by Mr. Sewall it appears that 1200 to 2175 lb. of the resistance was probably due to the friction caused by the internal fluid test pressure and of the remaining 5440 to 9860 lb., 4950 to 8960 lb. is what could be expected from a 3-inch twelve gage tube.

8 This again brings the comparable figures well within the range between curves B & C in Fig. 2. Probably the harder rolled tubes

leaked before a slip of 2/100 occurred. It seems evident that direct comparison of figures should not be made from tests of 3-inch twelve gage tubes under a direct push and from tests of 3¼ inch eleven gage tubes under hydraulic pressure.

9 With proper corrections however, the two sets of experiments seem to agree very well. In Fig. 8 and Fig. 9 the cut showing the form of joint is diagrammatic only and purposely distorted to aid the eye in finding the several tests. The proper dimensions are all given so that the true form of the joint is disclosed, but had the drawing been to scale the detail would have been too small to be distinctive. There was no thickening of the tube walls.

10 The test figures given by Mr. Parker when plotted give a load-slip curve of the same character as those shown in Fig. 10, and the values at the slipping point when corrected for thickness, diameter and fluid pressure are comparable with the values found for the same form of joint in test No. 1, 2 and 4. It appears that leakage actually occurred with a slip of about 0.025 in. even with a seat one inch wide and a smooth hole. In fact this test and a reasonable inference from the B & W tests seem to show that leakage does occur with a very small disturbance of the original seating of the tube although the hole may be a smooth machined one.

11 Professor Benjamin raises a very pertinent question as to whether it is not better to have the point of weakness localized at the tube ends where leakage will give so timely a warning. While the factor of safety of the ordinary joint is ample for usual cases yet, as pointed out in the paper, there are stresses due to temperature problems not readily computed and which in some cases make a stronger joint desirable.

THE TOTAL HEAT OF SATURATED STEAM

By Dr. Harvey N. Davis, Published in The Journal for November

ABSTRACT OF PAPER

It has for some time been suspected that Regnault's formula for the total heat of saturated steam,

$$H = 1091.7 + 0.305\ (t - 32)\ \text{B.t.u.}$$

is considerably in error. This conclusion is confirmed by computing H above 212 deg., in terms of H_{212}, from the throttling experiments of Grindley, Griessmann and Peake, and the direct specific heat determinations of Knoblauch and Jakob. The result is

$$H = H_{212} + 0.3745\ (t - 212) - 0.000550\ (t - 212)^2$$

The best value of H_{212} seems to be 1150.3 B.t.u. The range of the new formula is from 212 deg. to about 400 deg. The greatest error in Regnault's formula in this range is 6 B.t.u. at 275 deg., but if extrapolated to higher temperatures the error in it increases very rapidly.

Below 212 deg. the observations of Dieterici, Smith, Griffiths, Henning and Joly show a thoroughly satisfactory agreement among themselves, and prove that Regnault's formula runs high, the error reaching 18 B.t.u. at 32 deg. There are corresponding errors on the specific volume values ordinarily used.

DISCUSSION

Prof. C. H. Peabody. This paper is so complete and conclusive that it needs no discussion; rather it is to be accepted as the most valuable contribution to the science and practice of steam engineering since the determination of the mechanical equivalent of heat by Rowland.

2 To my mind this piece of work, which cannot be appreciated too highly, emphasizes two features, first, that no good scientific work is ever wasted, and second, that the highest scientific ability is required to interpret and apply experimental data. It would add to the value of the paper if the author would append the references to the authorities quoted, somewhat more fully than he has done.

3 In consequence of the information presented by Dr. Davis it will be necessary to recompute our steam tables; in fact his paper informs us that a new table is to be published, which I am sure will be welcomed by engineers.

4 It may, however, be pointed out that existing tables are in error only to the extent of half of one per cent for the middle range of temperatures, and that such errors will give engineers but little concern, however distasteful they are to the computers of such tables.

5 But our temperature-entropy diagram and the temperature-entropy tables (for which I am responsible) need change in only one feature and that the one of least importance.

6 To show that this is true let us consider the usual expression for entropy of wet steam,

$$\frac{xr}{T} + \theta$$

x = quality or dryness factor.
r = heat of vaporization.
T = absolute temperature.
θ = entropy of the liquid.

7 In computations for a temperature-entropy diagram or table we begin by assigning some desired value to the entropy which remains constant for a given abscissa or column. Then

$$\frac{xr}{T} + \theta = \phi$$

$$\therefore\ xr = (\phi - \theta)\,T$$

which determines the product xr for any temperature and entropy even though the factors x and r should be unknown. Consequently the heat contents

$$xr + q = (\phi - \theta)\,T + q$$

will not be affected by changes in r.

8 Again the usual manner of computing the specific volume of saturated steam is by the equation

$$s = \frac{r}{A\,T}\,\frac{1}{\frac{dp}{dt}} + \sigma = n + \sigma$$

s = specific volume of saturated steam.
A = reciprocal of Joules equivalent.
$\frac{dp}{dt}$ = slope of temperature-pressure curve.
σ = specific volume of water.
u = increase of volume due to vaporization.

9 Now the specific volume of wet steam is

$$v = xu + \sigma = \frac{xr}{AT} \frac{1}{\frac{dp}{dt}} + \sigma$$

or substituting for xr its value

$$v = \frac{(\phi - \theta)\,T}{AT} \frac{1}{\frac{dp}{dt}} + \sigma$$

$$\therefore\ v = \frac{\phi - \theta}{A} \frac{1}{\frac{dp}{dt}} + \sigma$$

which shows that the specific volume for a given temperature and entropy will not be changed by a change in r.

10 On the contrary the quality or dryness factor

$$x = (\phi - \theta)\,\frac{T}{r}$$

depends directly on r.

11 These equations are deduced to show that of the several properties given on a temperature-entropy diagram or table only one, namely the quality, needs revision. The fact that the initial value of this factor is seldom known to the degree of certainty represented by half of one per cent has no particular bearing on this discussion unless it makes engineers somewhat impatient concerning it.

Prof. William D. Ennis. Professor Peabody has said what was fitting, and what he could most appropriately say, regarding this masterly paper. When the aims and methods of pure science can be as helpfully presented to engineers as Dr. Davis has presented them, we must derive inspiration. Two questions immediately arise in reviewing these revised values for the total heat of steam. First, are they of considerable engineering importance? Second, are the new

values final? On the first point: Even if we take H_{212} at 1150.3 the difference between the new and old values within ordinary ranges is small. Take, for example, an engine test showing a thermal efficiency of 0.1500, using saturated steam at 150 lb. pressure. The old and the new values of H are respectively 1191.9 and 1193.4; a difference which would make the thermal efficiency, based on the new value, read 0.1498. At the same time, the new values differ from the old to such an extent as to promise some noticeable variations in our steam tables.

2 As to the finality of Dr. Davis' deductions, it seems unquestioned that throttling methods for the determination of H are better than the older method, *provided* the values of C_p are accurately known and there is no question as to the relation between p and t at saturation. But are the values of C_p as yet established? Professor Heck has harmonized the two best sets of experiments and has regarded the question as "about settled." Dr. Davis also regards the question as settled, but in a different way; while Professor Thomas evidently holds it to be unsettled, because he is still experimenting. We cannot get final values of H until we have final values of C_p.

3 I am not quite clear as to whether the apparent check on Knoblauch's values of C_p is not after all in large measure an example of the circular fallacy. The analytical expression for C_p includes three terms. The first of these, $\frac{dH}{dt}$, may be taken from Regnault's formula or from the new formula; the value in the latter case depends quite directly upon C_p. The second term of the expression for C_p, $\frac{r}{T}$, also depends directly upon C_p, for $r = H - h$ and H has been computed from C_p. For the same reason, the third and last term also depends upon C_p, although the derivative $\left(\frac{dv}{dt}\right)_p$ may be obtained without regard to Knoblauch's values for C_p. The computed values of C_p thus depend, though not simply, upon the values assumed for C_p in the first place. We could, of course, obtain a great variety of curves like that suggested by the small circles in Fig. 6, according to the origin of our values of H and $\left(\frac{dv}{dt}\right)_p$. I have found, for example, using Regnault's values for H, p, t and Wood's formula of relation between p, v and t for the derivative, at 140 lb. absolute pressure, that $C_p = 0.622$; a value rather closer to Knoblauch's than Dr. Davis' computation would give. This strikes one as being purely accidental.

4 With correct values of C_p, there seems to be no possible objection to the accuracy of re-computing H by the proposed method. The best check on the whole work would be, then, to finally determine H directly by some appropriate method.

PROF. ROBERT C. H. HECK. I have not been able to give this subject the amount of consideration which it ought to have, as a preliminary to close quantitative criticism; but several points have occurred to me as worthy of general remark.

2 Dr. Davis, having made a close study of the data along this line, is highly competent to express an opinion as to how nearly the data which he has used are to be accepted as final. We may well accept his conclusion that further changes in the determined values of the specific heat of superheated steam will not produce any great changes in his total heat. Here the word "great" is used from the view-point of scientific precision, not in the engineering sense; and from this point of view the errors in Regnault's formula are very great. But whether the range of probable error, or of uncertainty, is as narrow as Dr. Davis thinks, is to my mind rather doubtful.

3 One fact that has been brought out in all the more useful experiments, especially those of Knoblauch and Jakob, is the extreme difficulty of actual physical realization of that state of steam, so simple in idea, known as dry saturation. This shows up very clearly when Regnault's values are plotted for comparison (Fig. 3 of the paper); consistently, his results fall below the others, indicating the probability, which has frequently been remarked, that his steam was not really dry.

4 Knoblauch and Jakob put their steam through a preliminary superheater, of the form of a long vertical cylinder, with a succession of "pine-tree" radiators, made of glass tubes on metal frames, and with the electrical conductor coiled on the glass tubes; current could be passed through as many as desired of these radiators, and the rest left dead; and the temperature of the current of steam could be measured at the dividing plane between each pair of radiators. It was found that the steam rose in temperature in passing the successive active heaters, but in the dead range it at first dropped off a little and then settled to uniformity from point to point along the line of flow. The drop after leaving the region of heat-impartation shows that sensible heat was being taken up in the steam-current, as by evaporation; and was explained as indicating the presence of

moisture or of saturated steam in the body of quite highly superheated steam, until sufficient time, with thorough mechanical mixture, had produced homogeneity.

5 In the experiments of Professor Thomas, the steam was passed through a number of small holes (in effect, tubes), where heat was imparted to it by electrical heater-coils. In one experiment the steam was just brought to the point where any more heat would make the temperature begin to rise above that of saturation; in the next, the steam was heated to some higher point; and the difference in energy consumed was the heat for superheating from saturation. Aside from any question as to accuracy in observation and in measuring instruments, it is legitimate to be doubtful, first, whether the steam is homogeneously dry saturated in Experiment A; second, whether it is homogeneously superheated in Experiment B.

6 In the throttling calorimeter, the steam at first flows through the orifice in practically adiabatic expansion, some of it being condensed in the operation; then, as the jet comes to rest in the low-pressure chamber, the kinetic energy gained in that first operation is changed back to heat, and the body of steam is thereby superheated.

7 Now the important question is, may we safely assume that the steam in the low-pressure section of the throttling calorimeter is homogeneous when its temperature is measured? The best case for the defense is made when a porous plug is used instead of an orifice, as in the experiments of Griessmann. In general, though, the probabilities appear to be more against the throttling method than against that of Thomas. Under the excellent work which Dr. Davis has done lies this uncertainty as to the inherent reliability of his data.

8 I turn now to the question of the specific heat of superheated steam near the saturation limit—assuming that this limit exists as a sharply defined line, and can be experimentally realized. In the paper which I presented at the Detroit meeting, an attempt was made to combine the results of the best experiments to date. The most uncertain thing about the operation of superheating was the starting point; but I had to have something to start from, and so what seemed the best and most intelligent guess was made. It was, to a considerable degree, however, just a guess, although with the redeeming feature that the resulting uncertainty was much less than the probable error in the total heat up to saturation. In the condition of the data, the best that could be aimed at was essential correctness for engineering purposes, with a judicious balancing of

indications and probabilities. The present paper steps upon a higher plane; and with its results fully confirmed, we shall be ready to go out into the region of superheat and really "get things down fine."

9 There is one idea to which I must again take exception, and this is the assumption that the initial specific heat of superheated steam under constant pressure must rise to infinity at the so-called critical temperature, 689 deg. fahr. Infinite specific heat is the characteristic of the ordinary mixture of steam and water, because such a mixture can absorb heat at constant pressure without rise of temperature. This property disappears, however, at the beginning of the critical state; and when it has disappeared from its proper habitat, to import it into the foreign region across the boundary appears to be rather unjustifiable.

10 One point further is to be noted, which even yet is, however, of little more than theoretical interest. The total heat which remains constant in a throttling operation is not quite the same as that which was measured by Regnault in his calorimeter and which is given in our steam tables. In the throttling calorimeter, what we may call the work of the feed-pump is included in the total heat. This total heat, which remains constant in the ideal case of no-radiation, comprises not only the internal or intrinsic energy, but also the external energy of expansion under constant pressure, measured by the product of pressure by volume. In Regnault's experiments, steam was generated in a little boiler, and passed at once into a calorimeter, where it was condensed and cooled, the whole operation taking place under full pressure: then the heat gotten out of the steam and measured was, in intent, just what is put into the steam in the ordinary boiler, but did not include the work done by the feed-pump in forcing the water into the boiler. Until our experimental data are much more reliable than any now available, this small difference remains of theoretical rather than practical importance: but it enters into every precise expression for the energy of the steam-jet, and must be taken into account in calculations.

Prof. Lionel S. Marks. For a long time it has been evident that Regnault's values for the total heat of saturated steam required some revision. Particularly is this true for steam of low pressure. Forty years ago Herwig[1] pointed out that the values of the total

[1] Herwig Pogg. Ann. Vol. 137, 19, 592. 1869.

heat below 120 deg. fahr. were all too low. In his low pressure experiments, Regnault's method of measuring the temperature of the evaporating water by the vapor pressure in the condenser, has very properly given rise to criticism. At higher temperatures the break in the experimental results is clear evidence of the existence of some notable error.

2 But it is not only on the score of inaccuracies in his determinations that Regnault's work has been subjected to adverse criticism. Many of the students of his work who have accepted as correct his experimental results, have found themselves unable to accept his interpretation of his results by a straight line law connecting total heats and temperatures. If the observations above 178 deg. cent. are set aside (on account of the trouble with the apparatus at that temperature) it will be seen from Fig. 3 that his points do not lie on a straight line—that they lie on a curve which resembles closely the Davis curve. Several physicists in recent years have found that a second degree equation gives the best representation of the relation of total heats and temperatures found by Regnault.

Thus Wüllner[1] proposes

$$\lambda = 589 \quad + \ .6003\, t - \ .001246\, t^2$$

Ekholm[2] gives

$$\lambda = 596.75 + 0.4401\, t - 0.000634\, t^2$$

and Starkweather[3] finds from Regnault's observations

$$\lambda = 603.2 \quad + .356\, t \quad - .00021\, t^2$$

for temperatures above 100 deg. cent., and

$$\lambda = 598.9 \quad + .442\, t \quad - .00064\, t^2$$

for temperatures below 100 deg. cent. (These equations are for 15 deg. calories and centigrade degrees.)

3 In his investigations of other liquids Regnault gave second degree equations for the relation between total heat and temperature in almost every case. It was the results of his trouble with his apparatus at 178 deg. cent. that forced him to give a straight line relation between the total heat and temperature of saturated steam. It will be seen that the Davis equation representing the relation

[1] Wüllner Lehrbuch der Experimentalphysik. Vol. 2, 773. 1896.

[2] Ekholm, Fortschritte der Physik. Vol. 46 II, p. 371.

[3] Starkweather, Am. Jour. of Science (4) Vol. 7, p. 13. 1899

between the total heat of steam and its temperature between 212 deg. and 400 deg. fahr. and based upon the work of a number of modern investigations is of the same form as those given by the most recent analysis of Regnault's work.

4 Other equations have been proposed in recent years based entirely or partly upon other investigations than those of Regnault. Dieterici[1] has deduced an equation for r based on his own experimental value of 0 deg. cent. on Regnault's work, and on certain theoretical and empirical conclusions. The equation is,

$$r = 5948 - 0.559t - 0.000{,}002{,}234t^2$$

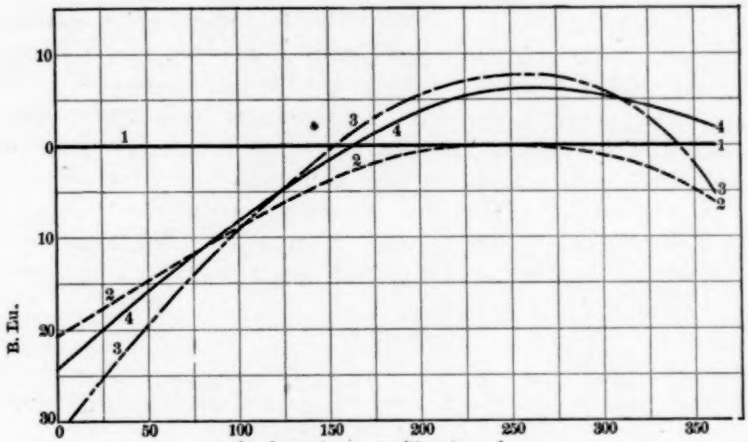


Fig. 1 Variation for Regnault's Values of r Curve 1–1, Regnault; 2–2, Thiesen; 3–3, Linde; 4–4, Davis.

and it shows the same kind of deviation from Regnault's straight line law as the other cited equations. One of the most prominent is that of Thiesen[2] which gives an expression for the value of the latent heat of vaporization of any liquid in terms of the critical temperature, T_c,

$$r = k\,(T_c - T)^{\frac{1}{3}}$$

for water,

$$\log_{10} k = 1.924, \text{ and } T_c = 638 \text{ deg. cent.}$$

[1] Dieterici, Z.V.D.I., 49 (1905), p. 362–7.

[2] Thiesen, Verh. des Phys. Ges. zu Berlin, 1897–8.

Besides this Linde[1] has deduced the values of the latent heat of vapori zation from the specific volume determinations of Knoblauch, Linde and Klebe by the Clapeyron equation

$$r = A\,T.\frac{dp}{dt}\,.\,u$$

The relation between the values of r, as found by Regnault, by Thiesen, by Linde and by Davis are shown in Fig. 1. (Below 212 deg. fahr. the Linde curve is continued through the experimental points of Griffiths and Dieterici.) It will be seen that the Thiesen, Linde and Davis curves show deviations from Regnault's values which have the same general characteristics.

5 Above 400 deg. fahr. there are no reliable experimental observations. If the Davis formula were assumed to be true for temperatures above 400 deg. fahr. it would lead to a maximum value of the total heat at 552 deg. fahr. The critical temperature is 689 deg. fahr. There is no direct experimental evidence to show that the total heat goes through such a maximum and deductions from characteristic equations cannot be used, as they, of necessity, must be widely extrapolated to give any evidence in that region. The value of the total heat must however reach a maximum before the critical temperature.

6 It can be shown that at the critical temperature $\frac{dH}{dT}$ is negative. The isothermal for the critical temperature is generally assumed to be horizontal (on the pv plane) where it meets and is tangent to the steam dome. If that is so, the value of $\frac{dH}{dT}$ is minus infinity at the critical temperature. If the critical isothermal is not tangent to the steam dome where it meets it, it must be because the steam dome is not rounded on top but comes to a peak at the intersection of the water and saturated steam lines. In this case the value of $\frac{dH}{dT}$ at the critical temperature will be finite but still negative. In either case the value of $\frac{dH}{dT}$ is negative and must have gone through zero in approaching the critical temperature, or in other words the total heat, H, must have gone through a maximum. Just where that maximum value occurs, there is no direct evidence to show either for water or for any other liquid. It is probable that the maximum

[1] Linde. Mitt. über Forschungsarbeiten, 1905, No. 21, p. 71

value does not occur as far from the critical temperature as an extrapolation of the Davis formula would indicate, though there is no experimental evidence to support this opinion.

7 The method that has been used in this paper for finding the variation of the total heat of saturated steam with the temperature is a method capable of giving very accurate results. The remarkable agreement of the results from the three separate sets of throttling experiments is valuable evidence on that point. The accuracy depends, however, on the use of the proper values for the specific heat of superheated steam. As Dr. Davis has pointed out there is very

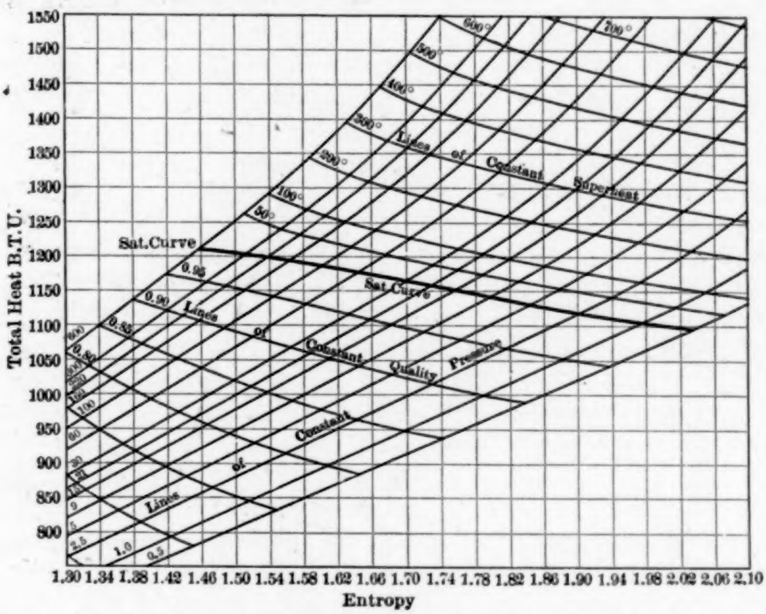


FIG. 2 MOLLIER DIAGRAM SHOWING TOTAL HEAT AND ENTROPY OF STEAM, BASED ON TOTAL HEAT VALUES OF DR. DAVIS

little question as to the correctness of the Knoblauch values of specific heat at low pressures and moderate superheats. For higher pressures and moderate superheat if the Knoblauch values are somewhat high (as the thermodynamically computed values of C_p at the saturation line, Fig. 6, would indicate) the effect will be to make the Davis total heat curve (Fig. 3) somewhat low at high pressures. It will be seen that the Davis curve goes close to the two Regnault (R) circles above 180 deg. cent. It is not known exactly what changes were made by Regnault in his apparatus after it had broken down at 178 deg. cent.,

but it is highly improbable that he succeeded in obtaining dry and saturated steam above that temperature. He certainly had not obtained it at lower temperatures and apparently was at no time cognizant of that fact. If there was no other source of error present the Regnault circles should be below the correct curve, *i.e.*, the Davis curve should lie above the Regnault circles.

8 At the present state of experimental knowledge of the specific heats of superheated steam the Davis curve seems to be the best that can be drawn. Ultimately, it will probably be found that it should

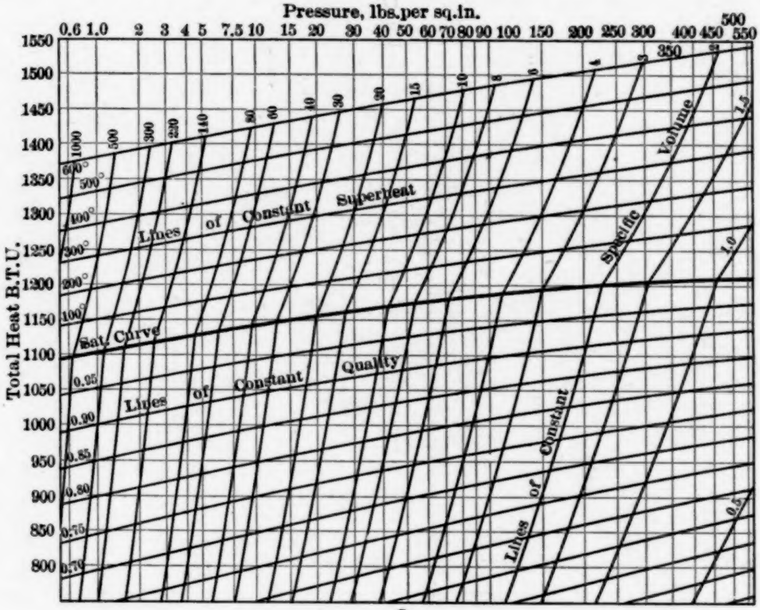


Fig. 3 Total Heat-Pressure Diagram Showing Specific Volumes, Based on Values of Dr. Davis

lie somewhat higher at high pressures and it is probable that an equation giving a maximum value of the total heat at a temperature considerably nearer to the critical temperature will represent the facts better than the equation proposed. I have had the opportunity of going over the work of Dr. Davis in considerable detail and in my opinion the claim he makes for his formula of an accuracy within one-tenth of one per cent between 212 deg. and 400 deg. fahr. is justified.

9 For the purpose of facilitating calculations involving saturated and superheated steam two diagrams (Fig. 2 and 3) have been pre-

pared. These diagrams have been plotted using the total heats of saturated steam given in this paper and the calculated corresponding values of entropy and specific volume. The specific volumes, entropies and total heats of superheated steam have been taken or calculated from the best modern data.

10 Of these two diagrams Fig. 2 is the total heat-entropy diagram devised by Professor Mollier, showing the total heat and entropy of steam in any condition and permitting the immediate determination of the work done in the Rankine unjacketed cycle; of the change in the condition of steam during adiabatic expansion or throttling: and also giving immediate information about wet steam of any usual quality. The other diagram, Fig. 3, is a total heat-pressure diagram showing specific volumes, qualities, and superheats. This diagram is plotted with pressures as abscissæ on a varying scale—equal distances along the axis of abscissae represent equal increments in the temperature of saturated steam corresponding to the indicated pressures. A scale of this kind has the advantage of spreading out the constant specific volume lines at the lower pressures. The second diagram (Fig. 3) is of the greatest value when problems involving volumes or ratios of expansion are to be solved. By the use of the two diagrams singly or together it is possible to solve a large number of commonly occurring problems in steam engine and steam turbine work—some of which problems can otherwise be solved only by a protracted series of trials and errors.

11 There is an apparently curious feature about these diagrams to which attention may be called. The lines of constant superheat are seen to diverge from the saturated steam line at high pressures. This of course results from the large specific heat of moderately superheated steam at high pressures. Such a divergence must necessarily take place. The total heat of saturated steam is tending to a maximum at some temperature below the critical temperature; the total heat of superheated steam along a line of constant superheat (and therefore of increasing pressure and temperature) does not pass through any maximum.

Prof. I. N. Hollis. This, perhaps, is one of the most important subjects that the mechanical engineer has to take up at present. There will not be great changes in Regnault's tables, but the paper points the way to greater scientific accuracy in the work of the mechanical engineer. Up to this time we have had so much work in developing the great projects American engineers have had to under-

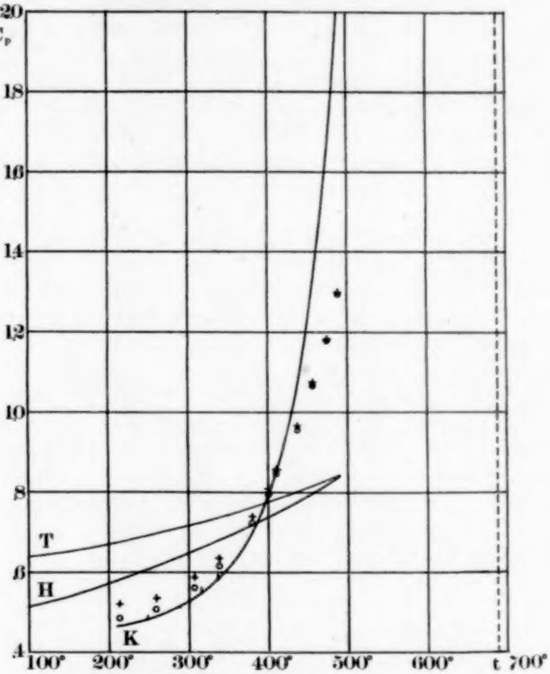
take, that we have paid more attention to the strictly practical side of questions and have permitted the electrical engineers to far surpass us in mathematical work given to the profession. So I welcome this paper by Dr. Davis, who, by the way, is a physicist at Harvard University and not an engineer, as it opens the way for greater accuracy in mechanical engineering matters.

THE AUTHOR. The friendly words both of approval and of criticism with which this paper has been received are much appreciated by the author. In particular Professor Peabody's intention to recompute his well-known steam tables on the basis of these new values is the pleasantest recognition which they could have. The ease with which this can be done for the wet steam part of his temperature-entropy diagram is most surprising, and it is unfortunate that equally simple laws do not hold for superheated steam. It is hoped that the lack of references which he criticises will be remedied by the accompanying partial bibliography of the subject.

2 The original paper should also have contained a statement as to the heat unit employed. Two such are available, the standard B.t.u., which is the heat required to raise one pound of water from 60 deg. fahr. to 61 deg. fahr., and the mean B.t.u. which is the 180th part of the heat required to raise a pound of water from the freezing point to the boiling-point. Of these, the second is better known in terms of mechanical or electrical units than the first, because the specific heat of water happens to be changing with temperature more rapidly near 60 deg. fahr. than elsewhere, so that the experimental determination of the standard B.t.u. is difficult and uncertain. An additional advantage of the mean B.t.u. is the simple conversion-factor ($\frac{5}{9}$) between steam tables based on it and those based on the Bunsen or mean calorie, now becoming standard abroad. Inasmuch as the difference between the mean B.t.u. and the 60 deg. B.t.u. is probably between one-thirtieth and one-tenth of one per cent, so that it makes practically no difference to the engineer which he uses, the mean B.t.u. has been used in this paper and in the steam tables which are to be based on it.

3 It seems necessary to emphasize again that the point of view of this paper is not so much that the question of the specific heat of steam is "about settled," as that it makes very little difference in the results of this paper whether it is settled or not, because *the computation method is extremely insensitive to errors in* C_p. To prove this, the total heat of saturated steam (H), has been recomputed

using Thomas' values which are at the opposite extreme of the C_p controversy from Knoblauch's. The result is to raise H by an amount which is zero at 212 deg., is well under a quarter of one per cent at 300 deg. and 67 lb. and is only about two-fifths of a per cent at 400 deg. and 250 lb. As was said in the paper, this is not an estimate of the probable error of the H formula given, for Thomas' values at low pressures and close to saturation are generally admitted to be too large. It is simply to show strikingly how small a difference in H is caused by comparatively large changes in C_p.



Reproduction of Fig. 6 of the Author's Paper, with Values Added of C_p from Thomas

4 Finally, the statement that the circularity of the reasoning at the end of the paper is apparent, not real, has been questioned. Let us, therefore, use the values of H, of r and of u obtained as above *from Thomas' values of* C_p as the basis of a recomputation of C_p by the method of Par. 19. The results are plotted with stars in the accompanying figure, which is otherwise a reproduction of Fig. 6 of the paper. The circles in it were obtained in the same way from

data based wholly on Knoblauch's values of C_p. It is evident that no matter what set of values we start from, the method leads in the end to practically the same curve. The fact that this curve is, in general, much more like Knoblauch's than like Thomas' would seem to be conclusive.

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[1] Of these only the first had been published when the computations for this paper were being made, but the new experimental data in the second paper agree almost perfectly between 212 deg. and 400 deg. with the values which were used.

GAS POWER SECTION

REPORTS AND DISCUSSIONS AT THE ANNUAL MEETING

In the January number of The Journal was published a brief account of the business session of the Gas Power Section, held during the Annual meeting of the Society, with the reports made by committees of the Section. As supplemental to this, there is here given the following report of the Executive Committee of the Section, which outlines a comprehensive plan for committee work; there is also appended discussion presented upon the Report of the Standardization Committee which has formed the subject for discussion at previous meetings as well.

EXECUTIVE COMMITTEE'S PROGRAM

Resolved; that the following committees be recommended for appointment

_a_ Nominating Committee.

- **(1) Canvass for candidates.**
- **(2) Place at least two candidates in nomination for each office, including standing officers and Executive Committee.**
- **(3) Committee to consist of nine men with not more than four in New York City.**

_b_ Tellers Committee.

- **(1) Three members, all in New York district.**

_c_ Library Committee.

- **(1) To keep track of bibliography and maintain two lists.**
  - **_a_ Publications in Library.**
  - **_b_ Publications which are not on file in Library.**
- **(2) Some member of the engineering magazine staff should be placed upon this committee.**
- **(3) The committee should contain men who can read:**
  - **_a_ French.**
  - **_b_ German**
  - **_c_ Spanish.**
  - **_d_ Italian, and possibly**
  - **_c_ Swedish.**
  - **_f_ Russian.**

(4) The Gas Power Section should provide copies of The Journal for the use of the reviewers.

(5) The Committee should make a bi-yearly report giving the lists of material in the library.

(6) The Committee to consist of 15 members, to be made up largely of men in editorial and professional work.

d Committee on Installations.

(1) To list all installations. For this purpose the committee should prepare a standard report form showing completely the equipment, capacities, etc., with dimensions. This committee should keep in touch with the manufacturers.

(2) The Committee to consist of three members.

e Committee on Plant Operation.

(1) Should prepare forms to show load curves, average, maximum, minimum, variations with the time of day, time of year, etc., or descriptions to cover these points in case the load curves cannot be plotted.

(2) Forms should also give the cost, character and amount of materials both in total and per unit, such as fuel, lubricating oil, batteries, current water gaskets, waste, cleaning, etc.

(3) Repair materials and costs.

(4) Operating labor, number and character of men and wages paid (total and per unit).

(5) Repair, labor, cost, etc.

Note: Where these items cannot be divided report in groups or in **totals**

(6) Detailed dimensions, etc., of the plant.

(7) Data on reliability of operation, that is, long runs.

(8) The Committee should consist largely of operating men.

(9) It is suggested that 20 men serve on this Committee.

f Committee on Accidents, Break-Downs and Failures.

(1) This committee to keep in touch with the liability companies, builders and operators, especially abroad, relating to both American and foreign equipment.

g Committee on Question Box.

(1) To conduct a correspondence on questions and answers and to publish complete lists of all questions and answers, as has been done by the National Electric Light Associations and the Gas Light Associations.

(2) Committee of five.

The Executive Committee would welcome suggestions from the membership regarding members for these various committees.

Affiliates of the Section are eligible to membership on these committees, and their hearty coöperation in the work is deemed extremely important.

Respectfully submitted,

R. H. Fernald
F. H. Stillman
G. T. Rockwood
F. R. Low

Executive Committee.

REPORT OF COMMITTEE ON STANDARDIZATION

DISCUSSION

Prof. Lionel S. Marks. In the discussion of the heat value of a gas the terms "lower" heat value and "effective" heat value have been used as synonymous. It seems to me a pity to introduce into gas engine practice the confusion which must result from this nomenclature. The two words have definite and different meanings in German practice. "Effective" heat value is the heat value of unit volume of a gas under the conditions of pressure and temperature at which the gas is used. The capacity of a gas engine depends on the effective heat value of the gas. At high altitudes a gas of given composition has a lower effective heat value and consequently an engine using it has diminished capacity. Effective heat value as defined above is a useful quantity and no other name has been proposed for that quantity. It is to be hoped that the committee will follow the German practice in the above respect. "Effective" heat values can, of course, be either "higher" or "lower" heat values.

2 There can be but little doubt that "lower" heat values give the better basis from which to measure the relative efficiencies of different gas engines. There is, however, some divergence in practice in the method of computing lower heat values. Many engineers find the lower heat value of a gas by subtracting from the higher heat value (as determined by the Junker calorimeter) the latent heat, at atmosphere pressure, of the steam that is formed by combustion. The British Institution of Civil Engineers in its code of rules finds the lower heat value by subtracting from the higher heat value the total heat of the steam at atmosphere pressure measured above water at 32 deg. fahr. The Verein deutscher Ingenieure takes as the lower heat value the heat which is liberated by the complete combustion of the fuel when the products of combustion are cooled to the original room temperature at constant pressure—it being assumed that all moisture present remains in the form of a vapor.

3 There is only one meaning of lower heat value which is logical. The use of the lower value is based upon the knowledge that at the temperatures existing in gas engine cylinders, the working substance

is a mixture of gases and highly superheated vapors. The fact that water vapor condenses after leaving the cylinder and therefore gives up more heat than would be the case if it remained a vapor, does not help the engine at all. Two exactly similar engines using fuels which differed from one another only in the matter of condensibility after leaving the cylinder ought to show the same efficiency on the lower heat basis. They will do so only if the lower heat value is calculated on the assumption that the water vapor remains a vapor down to the room temperature.

4 If one pound of dry and saturated steam at a temperature t_s is cooled at constant pressure to the room temperature t_r, it gives up $(r_s + q_s - q_r)$ B.t.u. This is the heat given up by the steam if "higher" heat-values are used.

5 If the dry and saturated steam could be kept a dry vapor when cooled at constant pressure to the room temperature it would give up

$$\int_{t_r}^{t_s} C_p\, dt \text{ B.t.u.}$$

This is the heat given up by the steam if "lower" heat-values are used. The difference between the higher and the lower values is consequently

$$w \left(r_s + q_s - q_r - \int^{t_s} C_p\, dt\right) \text{ B.t.u.}$$

where w is the weight of steam in the products of complete combustion of the gas.

6 The values of r_s and q_s cannot be found without considerable trouble as they depend upon the temperature at which the water vapor in the products of combustion becomes saturated. This temperature varies with the excess of air used for combustion. The condensation of water vapor in the Junker calorimeter takes place at all temperatures from the saturation temperature to the temperature of discharge of the gases. Consequently the value of $r_s + q_s$ can be stated approximately only. It is probable that the water vapor never forms as much as 15 per cent by volume of the products of combustion; that is, its partial pressure is not more than 2.2 lb. per sq. in. This corresponds to a saturation temperature of 130 deg.

fahr. Condensation of the water vapor would not begin until the products of combustion (at atmosphere pressure) fell to 130 deg. fahr. At that temperature $q + r = 1116$ B.t.u. If the final temperature of the products is 70 deg. we have for the lowest value $q + r = 11,098$ B.t.u. There cannot be any important error in assuming $q_s + r_s = 1110$ B.t.u. (corresponding to $t_s = 120$ deg. fahr.) for such condensation as occurs in a Junker calorimeter. To find the value of $\int_{t_r}^{t_s} C_p dt$ it will be simplest to assume C_p constant—its value averages about 0.46 under Junker calorimeter conditions. The difference between the higher and the lower heat values is then

$$w\,[r_s + q_s - (t_r - 32) - C_p\,(t_s - t_r)] \text{ B.t.u.}$$

and if t_r is 60 deg. fahr., this difference becomes

$$w\,(1110 - 28 - 0.46 \times 50) \text{ B.t.u.}$$
$$= w \times 1060 \text{ B.t.u., approximately.}$$

7 Where great accuracy is required it should not be forgotten that the Junker calorimeter gives the real higher heat value only

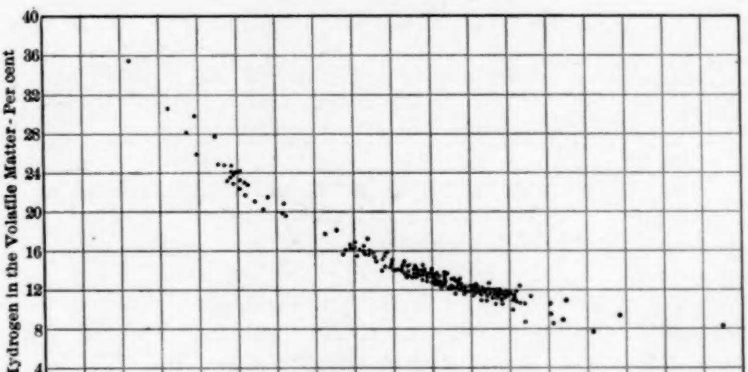

FIG. 1 RESULTS FROM ANALYSES OF 240 FUELS MADE FOR THE U. S. GOVERNMENT

in the case where the air and the gas are saturated with water vapor, where the products of combustion escape at the temperature of the room and where there is no contraction in volume. The importance of the correction for the non-realization of these conditions is shown by the fact that the operation of a Junker calorimeter with producer

gas very often gives no condensation water, that is, that the Junker calorimeter in that case is giving lower heat values. The correction is rather troublesome as it requires the analysis not only of the gas but also of the products of combustion coming from the calorimeter. From these analyses the relative volumes of air, gas, and products of combustion can be determined and if the humidity of the air and gas are also known the weight of uncondensed vapor escaping with the products of combustion can be determined.

8 If the efficiency of a producer is to be determined on a lower heat basis the hydrogen content of the coal must be known. This quantity can be determined experimentally only by carrying out the ultimate analysis of the coal—an analysis which is best made by

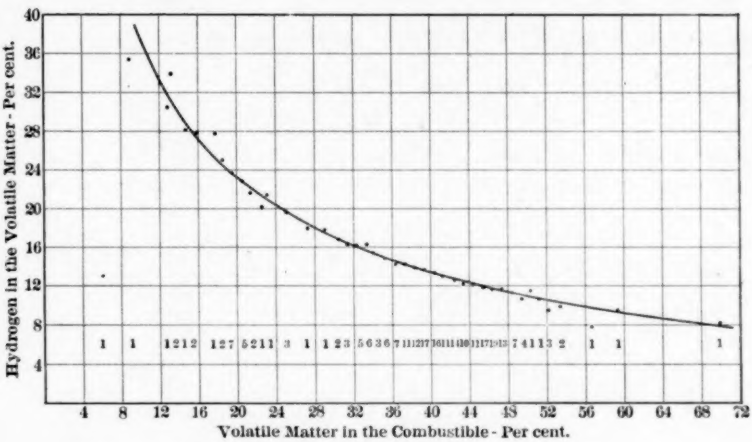

FIG. 2 CURVE PLOTTED FROM THE DIAGRAM IN FIG. 1

a chemist. In consequence of certain indications I have made an investigation of an apparent relation between the amount of volatile combustible matter in coal and the hydrogen content. The results of this investigation have just been published elsewhere.[1] They show that for the fuels occurring in the United States it is possible to determine the hydrogen content of the coal from the proximate analysis alone and with an accuracy that is sufficient for all practical purposes. Of 240 fuels analyzed, from every part of the United States and of every kind from anthracite to peat, only one fuel (a graphitic anthracite from Rhode Island with six per cent of volatile combustible), shows a hydrogen content differing by more than a negligible amount from that indicated by the proximate analysis.

[1] Power and the Engineer, Dec. 1, 1908.

9 In Fig. 1 are shown points representing the percentage of volatile matter in the combustible and the hydrogen content of that volatile matter for each of the 240 fuels of which analyses have been made and published by the United States government during the past four years. Many of these points are superposed. They represent solid fuels containing from 6 to 70 per cent volatile matter. It will be seen that these points represent a smooth curve very closely To draw the curve accurately the average position of the points has been calculated for each one per cent range of volatile matter and these average points are given in Fig. 2. The number below each point is the number of fuels of which it is the average. Over 90 per cent of the fuels tested have a volatile combustible matter between 8 and 48 per cent. The maximum difference between the curve and the individual points within that range represents a possible error of one-half per cent in the determination of the hydrogen in the coal. Above 48 per cent of volatile matter—that is with lignite and peat—there is possibility of an error of 1 per cent in the determination of the hydrogen of the fuel from the curve. One per cent error in the

TABLE 1

Per cent of volatile matter in the combustible	Per cent of Hydrogen in the combustible	Per cent of volatile matter in the combustible	Per cent of Hydrogen in the combustible
10	3.4	40	5.35
12	3.8	42	5.39
14	4.1	44	5.42
16	4.3	46	5.45
18	4.5	48	5.47
20	4.65	50	5.49
22	4.8	52	5.5
24	4.9	54	5.53
26	4.98	56	5.55
28	5.05	58	5.57
30	5.12	60	5.59
32	5.17	62	5.61
34	5.22	64	5.63
36	5.27	66	5.65
38	5.31	68	5.66
		70	5.67

hydrogen is equivalent to about seven-tenths of one per cent error in the determination of the lower heat value of a fuel. This is the limit of error by the use of the curve, Fig. 2, when lignite or peat is being used. For ordinary bituminous or anthracite coals the limit of error is about three-tenths of one per cent. The probability of error for fuels which are neither extremely high or extremely low in volatile matter is not more than one-tenth of one per cent.

10 In consequence of the above, I believe that engineers may safely omit the troublesome ultimate analysis of coal when they desire to know its hydrogen content and they may determine it from the usual proximate analysis.

11 The accompanying table gives the percentage of hydrogen in the combustible matter corresponding to various percentages of volatile matter in the combustible.

ACCESSIONS TO THE LIBRARY

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EXCHANGES

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Products and Facilities. (Bulletin No. 101.)
Surface Condensers. (Bulletin No. 102.)
Wheeler-Edwards Patent Air Pump. (Bulletin No. 103.)

EMPLOYMENT BULLETIN

The Society has always considered it a special obligation and pleasant duty to be the medium of securing better positions for its members. The Secretary gives this his personal attention and is most anxious to receive requests both for positions and for men available. Notices are not repeated except upon special request. Copy for notices in this Bulletin should be received before the 15th of the month. The list of men available is made up of members of the Society and these are on file, with the names of other good men not members of the Society, who are capable of filling responsible positions. Information will be sent upon application.

POSITIONS AVAILABLE

042 High-grade draftsman in the Isthmian service. Two vacancies in the position of general draftsman at an entrance salary of $125 a month. Appointees must have ability to handle estimates, all classes of railroad work, municipal engineering and masonry details.

043 General Foreman of Machine Shops, large electrical concern. Must have had similar position in machine shop or position of authority. Location, Chicago.

044 Master Mechanic in charge of tool equipment and design; expert machinist, good inventive ability and some general experience, preferred rather than special. Location, Chicago.

045 Engineer of Methods, for general investigation work on shop methods, materials, processes, etc. Technically trained man with experience on general investigation work. Man of general ability. Location, Chicago.

046 Head of installation-department for the erecting and installation of motors, generators and switchboards; to look after general repairs and complaints of service; capable of making reliable estimates of costs of work. Location, Chicago.

MEN AVAILABLE

203 Design of boilers, piping and pump layouts, foundations, and all work in connection with power station design. Charge of drafting room. Experienced in construction work, full charge in last position.

204 Member, technical graduate, six years in charge of office of draftsmen, designing automatic and special machinery. Experience as shop superintendent, sales-agent, representative abroad. Inventive ability.

205 Associate Member, technical graduate. Experienced in organizing, systematizing, estimating, inspecting machinery, fuel economy, familiar with modern systems of cost-keeping, capable of handling men and getting results on constructions, operating or in engineering department. Experience for the last eight years has been principally in steel plants.

206 Member, technical graduate, 31 years old, familiar with smelting furnaces, and mill construction desires a position as engineer, or manager of an engineering concern.

207 Mechanical engineer, member, technical graduate, 15 years designing, shop, and selling experience, on hoisting, conveying and transmission machinery, railroad, contractors, and foundry equipment, heavy machine and structural work. Responsible position desired. Good references.

208 Junior, graduate Stevens, would like a position with an engineering firm. Experience in drafting-room and shop.

209 Junior, M. E., graduate of Cornell University, with experience in general construction work and shop management, desires position in the vicinity of New York, in which such experience would be of value.

210 Junior member, aged 28, would like to engage with a consulting industrial engineer, auditor, or mechanical engineer, or take position as assistant to chief executive of a large engineering or industrial concern. Trained as machinist and traveling erecting man. Technical graduate with special work in economics and auditing. Experienced in cost-keeping and purchasing. Has done independent work for several large corporations designing methods to increase net efficiency.

211 Member, graduate of Mass. Inst. Technology, 20 years manufacturing and mechanical experience in textile plants, draftsman, mechanical and steam engineer, master mechanic, assistant superintendent and superintendent. Would prefer a position with some New England mill as mechanical superintendent or as Assistant to manufacturing Superintendent.

212 Junior, 29 years old, now technical editor of a leading engineering periodical, desires to resume active engineering work. Practical experience in power plant design and operation; in hoisting and conveying work.

213 A good machine man desires a position as chief draftsman or superintendent of a machine works, preferably paper machinery.

214 Junior member, four years shop experience in design, and construction of steam engines and general machinery, three years in charge of drafting departments; past five years designing and supervising construction of steam, gas and hydro-electric power plants.

215 Junior member, technical graduate, married, age 35, ten years experience as chief engineer and salesman; design of special machinery and testing work, desires change. Broad experience through travel for special machinery and plant equipments. Will consider equitable proposal from a reliable company as sales manager in branch office, preferably in Southern territory.

216 Wanted, position, preferably with consulting or operating engineers within 100 miles of Philadelphia; Junior Member; M.E., Univ. of Penn.; five years experience in shops and design, inspection and testing of boilers, piping, and power plant apparatus.

217 Member, thoroughly practical mechanic, experienced in operation, erection, design, estimating, and sales; district representative or manager. Specialty, power plant, reciprocating and turbine engines, power transmission appliances

218 Member of the Society, with large experience in executive positions, street railway and electric lighting properties will be pleased to correspond with firms having headquarters in New York. Will invest up to ten thousand dollars.

219 Mechanical and civil engineer, University of Virginia, twenty-three years experience in various branches of engineering, ten years designing and installing hydro-electric power plants. Position desired along similar lines.

220 Member desires to correspond with a concern needing a man thoroughly experienced in the organization and equipment of the up-to-date manufacturing machine shop and foundry.

221 Mechanical engineer, Stevens graduate, ten years experience in shop work and manufacturing operations, plant construction and maintenance, specializing in chemical works. Broad experience in the economic handling of help. Desires position where training would be valuable.

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DONNELLY, James A. (Affiliate, 1908), Heating Engr., 132 Nassau St., New York, N. Y.
GALLUP, David Lamprey (Affiliate, 1908), Instr. in Mech. Engrg., Worcester Poly. Inst., Worcester, Mass.
GOEBBELS, Leonard V. (Affiliate, 1908), Mech. Engr., N. E. Cor. 33d and Walnut Sts., Philadelphia, Pa.
MacVEAGH, George Day (Affiliate, 1908), Engr. Gas Dept., National Meter Co., and *for mail*, 808 De Kalb Ave., Brooklyn, N. Y.
PETERS, Edward (Affiliate, 1908), 4042 Olive St., St. Louis, Mo.
ULBRICHT, Tomlinson C. (1908), Instr. Engrg. Dept., Pratt Inst., and *for mail*, 234 Willoughby Ave., Brooklyn, N. Y.

Student Branch

STEVENS INSTITUTE ENGINEERING SOCIETY

BACKER, L. H., 1909, 431 W. 6th St., Plainfield, N. J.
BADEAU, R. P., 1909, Stevens Inst., Hoboken, N. J.
BECK, G., Jr., 1909, 100 N. Maple Ave., E. Orange, N. J.
BECKWITH, C. F., 1909, 106 N. Clinton St., East Orange, N. J.
COBB, P. L., 1909, 38 Schermerhorn St., Brooklyn, N. Y.
DRAUDT, O. E., 1909, 38 Cambridge Pl., Brooklyn, N. Y.
EIDMANN, F. L., 1909, 80 Danielson St., Union Hill, N. J.
FORTMAN, E., 1909, 80 Hauxhurst Ave., Weehawken, N. J.
FREYGANG, G. G., 1909, 752 Boulevard Loop, Weehawken, N. J.
HOEXTER, S. J., 1909, 786 Cauldwill Ave., New York, N. Y.
LUDWIG, C. H., 1909, 804 Castle Point Terrace, Hoboken, N. J.
NYLAND, E., 1909, Pres., S. I. E. S., Stevens Inst. of Tech., Hoboken, N J.
PEASE, L. M., 1909, 94 Knickerbocker Rd., Englewood, N. J.
PRICE, T., 1909, 65 Newell Ave., Rutherford, N. J.
SIERADZKI, A., 1909, 222 W. 141st St., New York, N. Y.
SIEVERS, E. J. J., 1909, 65 Willow Ave., Hoboken, N. J.
VENNEMA, A. W., 1909, 185 Paulison Ave., Passaic, N. J.
BERGER, J. G., 1910, 18 Carlton Ave., Jersey City, N. J.
CADY, C. I., 1910, Walnut Terrace, Bloomfield, N. J.
CARR, W. DeL., 1910, 15 W. 4th, Bayonne, N. J.
CAWLEY, H., 1910, 91 Broad St., Newark, N. J.
COOK, G. C., 1910, 62 Park Ave., E. Orange, N. J.
CRANE, F. L., 1910, 227 Rahway Ave., Elizabeth, N. J.
CURTIS, J. B., 1910, Stevens Inst., Hoboken, N. J.
CYPHERS, J. F., 1910, 168 Dodd St., East Orange, N. J.
DORER, O. H., 1910, 200 Stuyvesant Ave., Irvington, N. J.
FERGUSON, R. E., 1910, 60 S. Grove St., E. Orange, N. J.
FITZGERALD, C. J., 1910, Stevens Inst., Hoboken, N. J.
GUNKEL, F. H. Jr., 1910, 158 10th St., Hoboken, N. J.
HAYNES, H. H., 1910, 11 W. 94th St., New York, N. Y.
KASSANDER, A. R., 1910, 1350 Madison Ave., New York, N. Y.
MacKAY, C., 1910, 164 Jefferson Ave., Brooklyn, N. Y.
MESSNER, M., 1910, 106 W. 8th St., Bayonne, N. J.
OGDEN, N., 1910, 216 Summit Ave., Summit, N. J.
ROSCOE, A. P., 1910, Stevens Inst., Hoboken, N. J.
WHYTE, A. C., 1910, Ridgefield Park, N. J.

COMING MEETINGS

AËRONAUTIC SOCIETY

February 10, etc., evenings, weekly meetings, Automobile Club of America, W. 54th St., New York. Secy., Wilbur R. Kimball.

AMERICAN GAS POWER SOCIETY

April 27, quarterly meeting, Minneapolis, Minn. Secy., R. P. Gillette.

AMERICAN GEOGRAPHICAL SOCIETY

February 23, 29 W. 39th St., New York, 8 p.m. Acting Secy., Geo. C. Hurlbut, 15 W. 81st St.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

February 12, 33 W. 39th St., New York, 8 p.m. Secy., R. W. Pope.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

February 19, monthly meeting, Toronto Section. Secy. *pro tem.*, W. H. Eisenheis, 1207 Traders' Bank Bldg.

AMERICAN INSTITUTE OF MINING ENGINEERS

February, 29 W. 39th St., New York.

AMERICAN MATHEMATICAL SOCIETY

February 27, Columbia University, New York, February 27, San Francisco Section. General Secy., F. N. Cole, Columbia University.

AMERICAN SOCIETY OF CIVIL ENGINEERS

February 17, March 3, 220 W. 57th St., New York. Secy., C. W. Hunt.

AMERICAN SOCIETY OF MECHANICAL ENGINEERS

February 23, March 9, monthly meetings, 29 W. 39th St., New York, 8 p.m. Secy., Calvin W. Rice.

BLUE ROOM ENGINEERING SOCIETY

March 4, 29 West 39th St., New York. Secy., W. D. Sprague.

BOSTON SOCIETY OF CIVIL ENGINEERS

February 17, monthly meeting, Tremont Temple. Paper: Mechanical Refrigeration, H. M. Haven; March 17, annual meeting. Secy., S. E. Tinkham, 60 City Hall.

BROOKLYN ENGINEERS CLUB

February 11. Paper: Control and Management of the Highways of France, N. P. Lewis. Secy., J. Strachan, 197 Montague St.

CANADIAN CEMENT AND CONCRETE ASSOCIATION

March 1–6, Convention, Toronto, Ont. Secy., Alfred E. Uren, 62 Church St.

CANADIAN MINING INSTITUTE

March 3–5, annual meeting, Windsor Hotel, Montreal, Que. Secy., H. Mortimer-Lamb. Windsor Hotel.

CANADIAN RAILWAY CLUB

March 2, Windsor Hotel, Montreal, Que., 8 p.m. Secy., Jas. Powell, St Lambert, Montreal.

CANADIAN SOCIETY OF CIVIL ENGINEERS

February 11, electrical section; February 18, mechanical section; February 25, mining section; March 5, business meeting, 413 Dorchester St. W., Montreal, Que. Secy., Prof. C. H. McLeod.

CANADIAN SOCIETY OF CIVIL ENGINEERS, Manitoba Branch

February 19, March 5, semi-monthly meetings, University of Manitoba. Secy., E. Brydone Jack.

CANADIAN SOCIETY OF CIVIL ENGINEERS, Toronto Branch

February 25, regular meeting, 96 King St. W. Secy., T. C. Irving, Jr.

CAR FOREMEN'S ASSOCIATION OF CHICAGO

March 8. Secy., Aaron Kline, 326 N. 50th St.

CENTRAL ASSOCIATION OF RAILROAD OFFICERS

March 9, Cincinnati, Ill.

CENTRAL ASSOCIATION OF RAILROAD OFFICERS

March 1, Indianapolis, Ind. Secy., G. B. Staats, Care Penna. Lines.

CENTRAL ASSOCIATION OF RAILROAD OFFICERS

March 8, Kansas City, Mo. Secy., F. H. Ashley, Gumbel Bldg.

CENTRAL ASSOCIATION OF RAILROAD OFFICERS

February 11, Toledo, O. Secy., H. M. Ellert.

CENTRAL RAILWAY AND ENGINEERING CLUB OF CANADA

February 16, Rossin House, Toronto, Ont. Paper: Y. M. C. A. Work, J. M. Dudley. Secy., C. L. Worth, Room 409, Union Sta.

CLEVELAND ENGINEERING SOCIETY

March 9, monthly meeting, Caxton Building. Secy., Joe C. Beardsley.

COLORADO SCIENTIFIC SOCIETY

March 6, monthy meeting, Denver. Secy., Dr. W. A. Johnston, 801 Symes Bldg.

ENGINEERING ASSOCIATION OF THE SOUTH

February 16, monthly meeting, Nashville Section, Carnegie Library Bldg. Section Secy., H. H. Trabue, Berry Blk., Nashville.

ENGINEERING SOCIETY OF THE STATE UNIVERSITY OF IOWA

March 2, monthly meeting, Iowa City, Ia. Secy., Dean Wm. G. Raymond.

ENGINEERS' AND ARCHITECTS' CLUB

February 15, 303 Norton Bldg., Louisville, Ky. Secy., Pierce Butler.

ENGINEERS' CLUB OF BALTIMORE

March 6, monthly meeting. Secy., R. K. Compton, City Hall.

ENGINEERS' CLUB OF CENTRAL PENNSYLVANIA

March 2, monthly meeting, Gilbert Bldg., Harrisburg. Secy., E. R. Dasher.

ENGINEERS' CLUB OF CINCINNATI

February 18, 25 E. 8th St. Secy., E. A. Gast, P. O. Box 333.

ENGINEERS' CLUB OF PHILADELPHIA

February 20, March 6, 1317 Spruce St. Secy., H. G. Perring.

ENGINEERS' CLUB OF TORONTO

February 11, etc., weekly meetings, 96 King St. W., Toronto, Ont. Secy., R. B. Wolsey.

ENGINEERS' SOCIETY OF MILWAUKEE

February 10, March 10, 456 Broadway, Milwaukee, Wis. Secy., W. Fay Martin.

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA
February 15, monthly meeting. Paper: The Manufacture of Portland Cement, W. M. Kinney. Secy., E. K. Hiles.

EXPLORERS' CLUB
March 5, 29 West 39th St., New York. Secy., H. C. Walsh.

ILLUMINATING ENGINEERING SOCIETY
February 11, monthly meeting, New York Section, 29 W. 39th St., 8 p.m. Secy., P. S. Millar.

INTERNATIONAL MASTER BOILER-MAKERS' ASSOCIATION
Spring of 1909, Convention in Louisville, Ky. Secy., Harry D. Vought, 95 Liberty St., New York. Standardizing of Blue Prints for Building of Boilers; Boiler Explosions; Best Method of Applying Flues, Best Method for Caring for Flues While Engine is on the Road and at Terminals and Best Tools for Same; Flexible Staybolts Compared with Rigid Bolts; Best Method of Applying and Testing Same; Steel vs. Iron Flues, What Advantage and What Success in Welding Them; Best Method of Applying Arch Brick; Standardizing of Shop Tools; Standardizing of Pipe Flanges for Boilers and Templets for Drilling Same; Which is the long way of the Sheet; Best Method of Staying the Front Portion of Crown Sheet on Radial Top Boiler to Prevent Cracking of Flue Sheet in Top Flange; Rules and Formulas; Senate Bill.

IOWA RAILWAY CLUB
February 12, Des Moines.

LOUISIANA ENGINEERING SOCIETY
March 8, annual meeting, 323 Hibernia Bldg., New Orleans. Secy., L. C. Datz.

MASSACHUSETTS STREET RAILWAY ASSOCIATION
February 10, March 10, Boston. Secy., Charles S. Clark, 70 Kilby St.

NATIONAL ASSOCIATION OF AUTOMOBILE MANUFACTURERS
March 3, New York. Secy., C. C. Hildebrand, 7 E. 42d St.

NATIONAL GAS AND GASOLINE ENGINE TRADES ASSOCIATION
February 9, Auditorium Hotel, Chicago, Ill. Secy., Albert Stritmatter, Cincinnati, O.

NEW ENGLAND ASSOCIATION OF GAS ENGINEERS
February 17, Boston, Mass. Secy., N. W. Gifford, 26 Central Sq., E. Boston.

NEW ENGLAND RAILROAD CLUB
February 9, Young's Hotel, Boston, Mass. Paper: Steel Rails, H. C. Boynton. Secy., Geo. H. Frazier, 10 Oliver St.

NEW ENGLAND STREET RAILWAY CLUB
February 25, American House, Boston, Mass. Secy., John J. Lane, 12 Pearl St.

NEW ENGLAND WATERWORKS ASSOCIATION
February 10, March 10, regular meetings. Secy., Willard Kent, Tremont Temple, Boston, Mass.

NEW YORK ELECTRICAL SOCIETY
February 17, 29 West 39th St., New York. Secy., G. H. Guy.

NEW YORK RAILROAD CLUB
February 19, 29 W. 39th St. Secy., H. D. Vought, 95 Liberty St.

NEW YORK SOCIETY OF ACCOUNTANTS AND BOOKKEEPERS
February 16, etc., weekly meetings, 29 W. 39th St., 8 p.m. Secy., T. L. Woolhouse.

NEW YORK TELEPHONE SOCIETY
February 16, 29 West 39th St., New York. Secy., T. H. Laurence.

NORTHERN RAILWAY CLUB
February 27, Commercial Club Rooms, Duluth, Minn. Secy., C. L. Kennedy.

NORTHWEST RAILWAY CLUB
March 9, St. Paul, Minn. Secy., T. W. Flannagan, Care Soo Line, Minneapolis.

NOVA SCOTIA SOCIETY OF ENGINEERS
February 11, monthly meeting, Halifax. Secy., S. Fenn.

PROVIDENCE ASSOCIATION OF MECHANICAL ENGINEERS
February 23, monthly meeting, Technical High School Hall, 8 p. m. Paper: Wireless Telegraphy, Walter W. Massie. June 22, annual meeting. Secy., T. M. Phetteplace.

PURDUE MECHANICAL ENGINEERING SOCIETY
February 17, etc., fortnightly meetings, Purdue University, Lafayette, Ind., 6:30 p.m. Secy., L. B. Miller.

RAILWAY CLUB OF PITTSBURGH
February 26, monthly meeting, Monongahela House, Pittsburgh, Pa., 8 p.m. Secy., J. D. Conway, Genl. Office, P. & L. E. R. R.

RENSSELAER SOCIETY OF ENGINEERS
February 12, etc., fortnightly meetings, 257 Broadway, Troy, N. Y. Secy., R. S. Furber.

ROCHESTER ENGINEERING SOCIETY
February 12, monthly meeting. Secy., John F. Skinner, 54 City Hall.

ST. LOUIS RAILWAY CLUB
February 12, monthly meeting, Southern Hotel. Secy., B. W. Frauenthal.

SCRANTON ENGINEERS' CLUB
February 18, Board of Trade Bldg. Secy., A. B. Dunning.

TECHNICAL SOCIETY OF BROOKLYN
February 19, March 5, bi-monthly meetings, Arion Hall, Arion Pl., Brooklyn, N. Y., 8:30 p.m. Pres., M. C. Budell.

TECHNOLOGY CLUB OF SYRACUSE
March 9, monthly meeting, 502 Bastable Blk., 8 p.m. Secy., Robert L. Allen.

WASHINGTON SOCIETY OF ENGINEERS
February 16, George Washington University, 8 p.m. Paper: Overflow Lands of the Yazoo Delta, V. H. Manning. Secy., John C. Hoyt, 1330 F St., N. W., Washington, D. C.

WESTERN RAILWAY CLUB
February 16, monthly meeting, Auditorium Hotel, Chicago, Ill., 8 p.m. Secy., Jos. W. Taylor, 390 Old Colony Bldg.

WESTERN SOCIETY OF ENGINEERS
February 12, Electrical Section; February 17, extra meeting; Paper: Manganese Steel, W. S. Potter. March 3, regular meeting; Paper: The Chicago Harbor and River, John M. Ewen. Secy., J. H. Warder, 1737 Monadnock Blk., Chicago.

OFFICERS AND COUNCIL

PRESIDENT

JESSE M. SMITH .... New York

VICE-PRESIDENTS

L. P. BRECKENRIDGE .... Urbana, Ill.
FRED J. MILLER .... Center Bridge, Pa.
ARTHUR WEST .... E. Pittsburg, Pa.

Terms expire at Annual Meeting of 1909

GEO. M. BOND .... Hartford, Conn.
R. C. CARPENTER .... Ithaca, N. Y.
F. M. WHYTE .... New York

Terms expire at Annual Meeting of 1910

PAST PRESIDENTS

Members of the Council for 1909

AMBROSE SWASEY .... Cleveland, O.
JOHN R. FREEMAN .... Providence, R. I.
FREDERICK W. TAYLOR .... Philadelphia, Pa.
F. R. HUTTON .... New York
M. L. HOLMAN .... St. Louis, Mo.

MANAGERS

G. M. BASFORD .... New York
A. J. CALDWELL .... Newburg, N. Y.
A. L. RIKER .... Bridgeport, Conn.

Terms expire at Annual Meeting of 1909

WM. L. ABBOTT .... Chicago, Ill.
ALEX. C. HUMPHREYS .... New York
HENRY G. STOTT .... New Rochelle, N. Y.

Terms expire at Annual Meeting of 1910

H. L. GANTT .... Pawtucket, R. I.
I. E. MOULTROP .... Boston, Mass.
W. J. SANDO .... Milwaukee, Wis.

Terms expire at Annual Meeting of 1911

TREASURER

WILLIAM H. WILEY .... New York

CHAIRMAN OF THE FINANCE COMMITTEE

ARTHUR M. WAITT .... New York

HONORARY SECRETARY

F. R. HUTTON .... New York

SECRETARY

CALVIN W. RICE .... 29 W. 39th Street, New York

EXECUTIVE COMMITTEE OF THE COUNCIL

Jesse M. Smith, *Chairman*
F. R. Hutton
Alex. C. Humphreys
Fred J. Miller
Frederick M. Whyte

STANDING COMMITTEES

FINANCE

Arthur M. Waitt (1), *Chairman*
J. Waldo Smith (3)
Edward F. Schnuck (2)
Robert M. Dixon (4)
Waldo H. Marshall (5)

MEETINGS

Willis E. Hall (1), *Chairman*
L. R. Pomeroy (3)
Wm. H. Bryan (2)
Charles E. Lucke (4)
H. de B. Parsons (5)

PUBLICATION

Arthur L. Williston (1),
H. F. J. Porter (3)
D. S. Jacobus (2) *Chairman*
H. W. Spangler (4)
Geo. I. Rockwood (5)

MEMBERSHIP

Henry D. Hibbard (1), *Chairman*
Francis H. Stillman (3)
Charles R. Richards (2)
George J. Foran (4)
Hosea Webster (5)

LIBRARY

H. H. Suplee (1)
Leonard Waldo (3)
Ambrose Swasey (2)
John W. Lieb, Jr. (4), *Chairman*
Chas. L. Clarke (5)

HOUSE COMMITTEE

Henry S. Loud (1), *Chairman*
Bernard Victor Swenson (3)
William Carter Dickerman (2)
Francis Blossom (4)
Edward Van Winkle (5)

Note.—Numbers in parentheses indicate length of term in years that the member has yet to serve.

SPECIAL COMMITTEES

1909

On a Standard Tonnage Basis for Refrigeration

D. S. Jacobus
A. P. Trautwein
G. T. Voorhees
Philip De C. Ball
E. F. Miller

On Society History

John E. Sweet
H. H. Suplee
Charles Wallace Hunt

On Affiliated Societies

F. R. Hutton, *Chairman*
R. H. Fernald
Alex. C. Humphreys
H. H. Suplee
F. W. Taylor

On Constitution and By-Laws

Chas. Wallace Hunt, *Chairman*
G. M. Basford
F. R. Hutton
D. S. Jacobus
Jesse M. Smith

On Conservation of Natural Resources

Geo. F. Swain, *Chairman*
Charles Whiting Baker
L. D. Burlingame
M. L. Holman
Calvin W. Rice

On International Standard for Pipe Threads

E. M. Herr, *Chairman*
William J. Baldwin
Geo. M. Bond
Stanley G. Flagg, Jr.

On Thurston Memorial

Alex. C. Humphreys, *Chairman*
R. C. Carpenter
Charles Wallace Hunt
J. W. Lieb, Jr.
Fred J. Miller

On Revision and Extension of the Code for Testing Gas Power Machinery

Charles E. Lucke, *Chairman*
E. T. Adams
Geo. H. Barrus
D. S. Jacobus
Arthur West

On Hudson-Fulton Celebration

Geo. W. Melville
M. L. Holman
Jesse M. Smith

SOCIETY REPRESENTATIVES
1909

On John Fritz Medal

HENRY R. TOWNE (1)
AMBROSE SWASEY (2)
F. R. HUTTON (3)
CHARLES WALLACE HUNT (4)

On Board of Trustees United Engineering Societies Building

CHAS. WALLACE HUNT (1)
F. R. HUTTON (2)
FRED J. MILLER (3)

On Joint Library Committee

THE CHAIRMAN OF THE LIBRARY COMMITTEE OF THE AM. SOC. M. E.

On National Fire Protection Association

JOHN R. FREEMAN
IRA H. WOOLSON

On Joint Committee on Engineering Education

ALEX. C. HUMPHREYS
F. W. TAYLOR

On Government Advisory Board on Fuels and Structural Materials

GEO. H. BARRUS
P. W. GATES
W. F. M. GOSS

On Advisory Board National Conservation Commission

GEO. F. SWAIN
JOHN R. FREEMAN
CHAS. T. MAIN

On Council of American Association for the Advancement of Science

ALEX. C. HUMPHREYS
FRED J. MILLER

NOTE.—Numbers in parentheses indicate length of term in years that the member has yet to serve.

OFFICERS OF THE GAS POWER SECTION

1909

CHAIRMAN

F. R. Low

SECRETARY

George A. Orrok

GAS POWER EXECUTIVE COMMITTEE

Prof. R. H. Fernald, *Chairman*
F. R. Hutton
G. I. Rockwood
F. H. Stillman
H. H. Suplee

GAS POWER MEMBERSHIP COMMITTEE

Robert T. Lozier, *Chairman*
D. B. Rushmore
George W. Whyte
Albert A. Cary
F. S. King

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Cecil P. Poole, *Chairman*
M. N. Latta
Chas. E. Sargent
F. P. Thorp
E. P. Coleman
C. L. Straub
W. T. Magruder
Arthur L. Rice